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ART. XXII.—*Thoughts on Species;* by JAMES D. DANA.

(Read before the American Association at Montreal, Aug. 13th, 1857.)

WHILE direct investigation of individual objects in nature is the true method of ascertaining the laws and limits of species, we have another source of suggestion and authority in the comprehensive principles that pervade the universe. The source of doubt in this synthetic mode of reaching truth consists in our imperfect appreciation of universal law. But science has already searched deeply enough into the different departments of nature to harmonize many of the thoughts that are coming in from her wide limits; and it is well, as we go on in research, to compare the results of observations with these utterings of her universality.

I propose to present some thoughts on species from the latter point of view, reasoning from central principles to the circumferential, and, if I mistake not, we shall find the light from this direction sufficiently clear to illumine a subject which is yet involved in doubts and difficulties.

The questions before us at this time are—

1. What is a species?
2. Are species permanent?
3. What is the basis of variations in species?

1. *What is a species.*

It is common to define a species as *a group* comprising such individuals as are alike in *fundamental* qualities; and then by

way of elucidation, to explain what is meant by fundamental qualities. But the idea of a group is not essential; and moreover it tends to confuse the mind by bringing before it, in the outset, the endless diversities in individuals, and suggesting numberless questions that vary in answer for each kingdom, class, or subordinate group. It is better to approach the subject from a profounder point of view, search for the true idea of distinction among species, and then proceed onward to a consideration of the systems of variables.

Let us look first to *inorganic* nature. From the study of the inorganic world we learn that each element is represented by a specific amount or law of force; and we even set down in numbers the precise value of this force as regards one of the deepest of its qualities, chemical attraction. Taking the lightest element as a unit to measure others by, as to their weights in combination, oxygen stands in our books as 8; and it is precisely of this numerical value in its compounds: each molecule is an 8 in its chemical force or law, or some simple multiple of it. In the same way there is a specific number at the basis of other qualities. Whenever then the oxygen amount and kind of force was concentered in a molecule, in the act of creation, the species oxygen commenced to exist. And the making of many such molecules instead of one, was only a repetition in each molecule, of the idea of oxygen.

In combinations of the elements, as of oxygen and hydrogen, the resultant molecule is still equivalent to a fixed amount, condition, or law, of chemical force; and this law, which we express in numbers, is at the basis of our notion of the new species.

It is not necessarily a different amount of force; for it may be simply a different state of concentration or different rate or law of action. This should be kept in mind in connection with what follows.*

The essential idea of a species, thence deduced, is this: a *species* corresponds to *a specific amount or condition of concentered force, defined in the act or law of creation.*

Turn now to the organic world. The individual is involved in the germ-cell from which it proceeds. That cell possesses certain inherent qualities or powers, bearing a definite relation to external nature, so that, when having its appropriate nidus or surrounding conditions, it will grow, and develop out each organ and member to the completed result, and this, both as to all

* When we have in view, oxygen and the elements, we are apt to think of their molecules as distinguished by a different *amount* and *kind* of force. But when we consider the many different compounds that may be made of the same elements (as carbon and hydrogen), in the very *same* proportions, we are led to conceive of these as differing molecularly in a different *law* of the same force or forces. When, again, we see the same element under conditions as diverse as any two compounds, as in cases of allotropism, we are still better satisfied with adopting, for the present, the most general expression—a different law of action or condition of molecular force.

chemical changes, and the evolution of the structure which belongs to it as a subordinate to some kingdom, class, order, genus and species in nature. The germ-cell of an organic being develops a specific result; and like the molecule of oxygen, it must correspond to a measured quota or specific law of force. We cannot apply the measure, as in the inorganic kingdom, for we have learned no method or unit of comparison. But it must nevertheless be true, that a specific predetermined amount, or condition, or law, of force is an equivalent of every germ-cell in the kingdoms of life. I do not mean to say that there is but one kind of force; but that whatever the kind or kinds, it has a numerical value or law, although human arithmetic may never give it expression.

A species among living beings, then, as well as inorganic, is based on *a specific amount or condition of concentered force defined in the act or law of creation.*

Any one species has its specific value or law of force; another, its value; and so for all: and we perceive the fundamental notion of the distinction between species when we view them from this potential stand-point. The species, in any particular case, began its existence when the first germ-cell or individual was created; and if several germ-cells of equivalent force were created, or several individuals, each was but a repetition of the other: the species is in the potential nature of the individual, whether one or many individuals exist.

Now in organic beings,—unlike the inorganic,—there is a cycle of progress involving growth and decline. The oxygen molecule may be eternal as far as any thing in its nature goes. But the germ-cell is but an incipient state in a cycle of changes, and is not the same for two successive instants; and this cycle is such that it includes in its flow, a reproduction, after an interval, of a precise equivalent of the parent germ-cell. Thus an indefinite perpetuation of the germ-cell is in fact effected; yet it is not mere endless being, but like evolving like in an unlimited round. Hence, when individuals multiply from generation to generation, it is but a repetition of the primordial type-idea; and the true notion of the species is not in the resulting group, but in the idea or potential element which is at the basis of every individual of the group; that is, the specific law of force, alike in all, upon which the power of each as an existence and agent in nature depends. Dr. Morton presented nearly the same idea when he described a species as *a primordial organic form.*

Having reached this idea as the starting point in our notion of a species, we must still, in order to complete and perfect our view, consider what is the true expression of this potentiality. For this purpose, we should have again in mind, that a living cell, unlike an inorganic molecule, has only a historical existence.

The species is not the adult resultant of growth, nor the initial germ-cell, nor its condition at any other point; it comprises the whole history of the development. Each species has its own special mode of development as well as ultimate form or result, its serial unfolding, inworking and outflowing; so that the precise nature of the potentiality in each is expressed by the line of historical progress from the germ to the full expansion of its powers, and the realization of the end of its being. We comprehend the type-idea only when we understand the cycle of evolution through all its laws of progress, both as regards the living structure under development within, and its successive relations to the external world.

2. *Permanence of species.*

What now may we infer with regard to the permanence or fixedness of species from a general survey of nature?

Let us turn again to the inorganic world. Do we there find oxygen blending by indefinite shadings with hydrogen or with any other element? Is its combining number, its potential equivalent, a varying number,—usually 8, but at times 8 and a fraction, 9, and so on? Far from this, the number is as fixed as the universe. There are no indefinite blendings of elements. There are combinations by multiples or submultiples, but these prove the dominance and fixedness of the combining numbers.

But further than this, fixed numbers, definite in value and defiant of all destroying powers, are well known to characterize nature from its basement to its top-stone. We find them in combinations by volume as well as weight, that is, in all the relations of chemical attraction; in the mathematical forms of crystals and the simple ratios in their modifications,—evidence of a numerical basis to cohesive attraction; in the laws of light, heat, and sound. Indeed, the whole constitution of inorganic nature, and of our minds with reference to nature, as Professor Peirce has well illustrated, involves fixed numbers; and the universe is not only based on mathematics, but on finite determinate numbers in the very natures of all its elemental forces. Thus the temple of nature is made, we may say, of hewn and measured stones, so that, although reaching to the heavens, we may measure, and thus use the finite to rise toward the infinite.

This being true for inorganic nature, it is necessarily the law for all nature, for the ideas that pervade the universe are not ideas of contrariety but of unity and universality beneath and through diversity.

The units of the inorganic world, are the weighed elements and their definite compounds or their molecules. The units of the organic are *species*, which exhibit themselves in their simplest condition in the germ-cell state. The kingdoms of life in all their magnificent proportions are made from these units. Were

these units capable of blending with one another indefinitely, they would no longer be units, and species could not be recognized. The system of life would be a maze of complexities; and whatever its grandeur to a being that could comprehend the infinite, it would be unintelligible chaos to man. The very beauties that might charm the soul would tend to engender hopeless despair in the thoughtful mind, instead of supplying his aspirations with eternal and ever-expanding truth. It would be to man the temple of nature fused over its whole surface and through its structure, without a line the mind could measure or comprehend.

Looking to facts in nature, we see accordingly every where, that the purity of species has been guarded with great precision. It strikes us naturally with wonder, that even in senseless plants, without the emotional repugnance of instinct, and with reproductive organs that are all outside, the free winds being often the means of transmission, there should be rigid law sustained against intermixture. The supposed cases of perpetuated fertile hybridity are so exceedingly few as almost to condemn themselves, as no true examples of an abnormity so abhorrent to the system. They violate a principle so essential to the integrity of the plant-kingdom, and so opposed to nature's whole plan, that we rightly demand long and careful study before admitting the exceptions.

A few words will explain what is meant by perpetuated fertile hybridity. The following are the supposable grades of results from intermixture between two species:—

1. No issue whatever—the usual case in nature.
2. Mules (naming thus the issue) that are wholly infertile whether among themselves or in case of connection with the pure or original stock.
3. Mules that are wholly infertile among themselves, but may have issue for a generation or two by connection with one of the original stock.
4. Mules that are wholly infertile among themselves, but may have issue through indefinite generations by connection for each with an individual of the original stock.
5. Mules that are fertile among themselves through one or two generations.
6. Mules that are fertile among themselves through many generations.
7. Mules that are fertile among themselves through an indefinite number of generations.

The cases 1 to 5 are known to be established facts in nature; and each bears its testimony to the grand law of purity and permanence. The examples under the heads 2 to 5 become severally less and less numerous, and art must generally use an unnatural play of forces or arrangements to bring them about.

Again, in the animal kingdom, there is the same aversion in nature to intermixture, and it is emotional as well as physical. The supposed cases of fertile hybridity are fewer than among plants.

Moreover, in both kingdoms, if hybridity be begun, nature commences at once to purify herself as of an ulcer on the system. It is treated like a disease, and the energies of the species combine to throw it off. The short run of hybridity between the horse and the ass, species very closely related, reaching its end *in one single generation*, instead of favoring the idea that perpetuated fertile hybridity is possible, is a speaking protest against a principle that would ruin the system if allowed free scope.

The finiteness of nature in all her proportions, and the necessity of finiteness and fixedness for the very existence of a kingdom of life, or of human science its impress on finite mind, are hence strong arguments for the belief that hybridity cannot seriously trifle with the true units of nature, and at the best, can only make temporary variations.

It is fair to make the supposition that in case of a very close proximity of species, there might be a degree of fertile hybridity allowed; and that a closer and closer affinity *might* give a longer and longer range of fertility. But the case just now alluded to seems to cut the hypothesis short; and moreover it is not reasonable to attribute such indefiniteness to nature's outlines, for it is at variance with the spirit of her system.

Were such a case demonstrated by well-established facts, it would necessarily be admitted; and I would add, that investigations directed to this point are the most important that modern science can undertake. But until proved by arguments better than those drawn from domesticated animals, we may plead the general principle against the *possibilities* on the other side. If there is a law to be discovered, it is a wide and comprehensive law, for such are all nature's principles. Nature will teach it not in one corner of her system only, but more or less in every part. We have therefore a right to ask for well-defined facts, taken from the study of successive generations of the interbreeding of species known to be distinct.

Least of all should we expect that a law, which is so rigid among plants and the lower animals, should have its main exceptions in the highest class of the animal kingdom, and its most extravagant violations in the genus Homo; for if there are more than one species of Man, they have become in the main indefinite by intermixture. The very crown of the kingdom has been despoiled; for a kingdom in nature is perfect only as it retains all its original parts in their full symmetry, undefaced and unblurred. Man, by receiving a plastic body, in accordance with a law that species most capable of domestication should necessa-

rily be most pliant, was fitted to take the whole earth as his dominion, and live under every zone. And surely it would have been a very clumsy method of accomplishing the same result, to have made him of many species, all admitting of indefinite or nearly indefinite hybridization, in direct opposition to a grand principle elsewhere recognized in the organic kingdoms. It would have been using a process that produces impotence or nothing among animals for the perpetuation and progress of the human race.

There are other ways of accounting for the limited productiveness of the mulatto, without appealing to a distinction of species. There are causes, independent of mixture, which are making the Indian to melt away before the white man, the Sandwich Islander and all savage people to sink into the ground before the power and energy of higher intelligence. They disappear like plants beneath those of stronger root and growth, being depressed morally, intellectually and physically, contaminated by new vices, tainted variously by foreign disease, and dwindled in all their hopes and aims and means of progress, through an overshadowing race.

We have therefore reason to believe from man's fertile intermixture, that he is one in species; and that all organic species are divine appointments which cannot be obliterated, unless by annihilating the individuals representing the species.

It may be said, that different species in the inorganic world combine so as to form new units, and why may they not in the organic? It is true they combine, but not by indefinite blendings. There is a definite law of multiples, and this is the central idea in the system of inorganic nature. In organic nature, such a law of multiples, if existing, would be general, as in the inorganic; it would be an essential part of the system and should be easily verified, while, in fact, observation lends it no support, not even enough to have suggested the hypothesis.

In one kingdom, the *inorganic*, there is multiplication of kinds of units by combination, according to the law of multiples, and no reproduction; while in the *organic*, there is reproduction of like from like and no multiplication of kinds by combination. And thus the two departments of living and dead nature widely diverge.

Neither does the possibility of mere mixture among inorganic substances afford any analogy to sustain the idea of possible hybrid mixture indefinitely perpetuated, among living beings. The mechanical aggregation of units that make up ordinary mixture, is one thing; and the combination that would alter a germ, one of the units in organic species, even to its fundamental nature, is quite another. This last is not aggregation. It is as different from mere mixture as is chemical combination and stands somewhat in the same relation, so that the analogy has no bearing on the question.

3. *Variations of Species.*

But there are variations in species, and this is our next topic. The principles already considered teach, as we believe, that each species has its specific value as a unit, which is essentially permanent or indestructible by any natural source of change; and we have, therefore, to admit in the outset, if these principles are true, that variations have their limits, and cannot extend to the obliteration of the fundamental characteristics of a species.

To understand these variations, we may again appeal to general truths.

Variation is a characteristic of all things finite; and is involved in the very conditions of existence. No substance or body can be wholly independent of every or any other body in the universe. The most comprehensive and influential law in nature, most fundamental in all change, composition or decomposition, growth or decay, is the law of mutual sympathy, or tendency to equilibrium in force through universal action and reaction.

The planets have their orbits modified by other bodies in space through their changing relations to those bodies. A substance, as oxygen or iron, varies in temperature and state of expansion from the presence of a body of different temperature; in chemical tendencies from the presence of a luminous body like the sun; in magnetic or electrical attraction from surrounding magnetic or electrical influences. There is thus unceasing flow and unceasing change through the universe. All the natural forces are closely related as if a common family or group, and are in constant mutual interplay.

The degree or kind of variation has its specific law for each element; and in this law the specific nature of the element is in a degree expressed. There is to each body or species, the normal or fundamental force in which its very nature consists; and, in addition, the relations of this force to other bodies, or kinds, amounts or conditions of force, upon which its variations depend. One great end of inorganic science is to study out the law of variables for each element or species. For this law is as much a part of an idea of the species, as the fundamental potentiality; indeed the one is a measure of the other.

So again, a species in the *organic* kingdoms is subject to variations, and upon the same principle. Its very development depends on the appropriation of material around it, and on attending physical forces or conditions, all of which are variable through the whole of its history. Every chemical or molecular law in the universe is concerned in the growth,—the laws of heat, light, electricity, cohesion, etc.; and the progress of the developing germ, whatever its primal potentiality, is unavoidably subject to variations, from the diversified influences to which it may be exposed. The new germ, moreover, takes

peculiarities from the parent, or from the circumstances to which its ancestry had been exposed during one or more preceding generations.

There is then a fixed normal condition or value, and around it librations take place. There is a central or intrinsic law which prevents a species from being drawn off to its destruction by any external agency, while subject to greater or less variations under extrinsic forces.

Liability to variation is hence part of the law of a species; and we cannot be said to comprehend in any case the complete idea of the type until the relations to external forces are also known. The law of variables is as much an expression of the fundamental equalities of the species in organic as in inorganic nature; and it should be the great aim of science to investigate it for every species. It is a source of knowledge which will yet give us a deep insight into the fundamental laws of life. Variations are not to be arranged under the head of *accidents;* for there is nothing accidental in nature; what we so call, are expressions really of profound law, and often betray truth and law which we should otherwise never suspect.

This process of variation, is the external revealing the internal, through their sympathetic relations; it is the law of universal nature reacting on the law of a special nature, and compelling the latter to exhibit its qualities; it is a centre of force manifesting its potentiality, not in its own inner working, but in its outgoings among the equilibrating forces around, and thus offering us, through the known and physical, some measure of the vital within the germ. It is therefore one of the richest sources of truth open to our search.

The limits of variation, it may be difficult to define among species that have close relations. But being sure that there are limits,—that science, in looking for law and order written out in legible characters, is not in fruitless search, we need not despair of discovering them. The zoologist, gathering shells or mollusks from the coast of eastern America and that of Japan, after careful study, makes out his lists of identical species, with the full assurance that species are definite and stable existences; and he is even surprised with the identity of characters between the individuals of a species gathered from so remote localities. And as he sees zoological geography rising into one of the grandest of the sciences, his faith in species becomes identified with his faith in nature and all physical truth.

If then we may trust this argument from general truths to special,—general *truths* I say, for general principles as far as established are truths—we should conceive of a species from the potential point of view, and regard it as—

a. A concentered unit of force, an ineffaceable component of the system of nature; but

b. Subject to greater or less librations, according to the universal law of mutual reaction or sympathy among forces.

And, in addition, in the *organic* kingdom,

c. Exhibiting its potentiality not simply or wholly in any existing condition or action, but through a cycle of growth from the primal germ to maturity, when the new germ comes forth as a repetition of the first to go another round in the cycle and perpetuate the original unit; and, therefore, as follows from a necessary perpetuity of the cycle—

d. Exhibiting identity of species among individuals, by perpetuated fertile intermixture in all normal conditions, and non-identity by the impossibility of such intermixture, the rare cases of continuation for one or two generations, attesting to the stability of the law, by proving the effort of nature to rid herself of the abnormity, and her success in the effort.

e. The many like individuals that are conspecific do not properly constitute the species, but each is an expression of the species in its potentiality under some one phase of its variables; and to understand a species, we must know its law through all its cycle of growth, and its complete series of librations.

We should therefore conceive of the system of nature as involving, in its idea, a system of units, finite constituents at the basis of all things, each fixed in law; these units in inorganic nature as adding to their kinds by combinations in definite propositions; and those in organic nature adding to their numbers of representative individuals, but *not* kinds, by self-reproduction; and all adding to their varieties by mutual reaction or sympathy. Thus from the law within and the law without, under the Being above as the Author and sustainer of all law, the world has its diversity, the cosmos its fullness of beauty.

I would remark again that we must consider this mode of reaching truth, by reasoning from the general to the special, as requiring also its complement, direct observation, to give unwavering confidence to the mind; and we should therefore encourage research with a willingness to receive whatever results come from nature. We should give a high place in our estimate to all investigation tending to elucidate the variation or permanence of species, their mutability or immutability; and at the same time, in order that appearances may not deceive us, we should glance towards other departments of nature, remembering that all truth is harmonious, and comprehensive law the end of science.

A word further upon our conceptions of species as realities. In acquiring the first idea of species, we pass, by induction, as in other cases of generalization, from the special details displayed among individuals to a general notion of a unity of type; and

this general notion, when written out in words, we may take as an approximate formula of the species. One system of philosophy thence argues that this result of induction is nothing but a notion of the mind, and that species are but an imaginary product of logic; or at least, that since, as they say (we do not now discuss this point), genera are groupings without definite limits which may be laid off variously by different minds, so species are undefined, and individuals are the only realities—the supposed limits to species being regarded as proof of partial study, or a consequence of a partial development of the kingdoms of nature. Another system infers, on the contrary, that species are realities, and the general or type idea has, in some sense, a *real* existence. A third admits that species are essentially realities in nature, but claims that the general idea exists only as a result of lŏgical induction.

The discussion in the preceding pages sustains most nearly the last view, that species are realities in the system of nature while manifest to us only in individuals; that is, they are so far real, that the idea for each is definite, even of mathematical strictness, (although not thus precise in our limited view,) it proceeding from the mathematical and finite basis of nature. They are the units fixed in the plan of creation; and individuals are the material expressions of those ideal units.

At the same time, we learn, that while species are realities in a most important and fundamental sense, no comprehensive type-idea of a species can be represented in any material or immaterial existence. For while a species has its constants, it has also its variables, each variable becoming a constant so far only as its law and limits of variation are fixed; and in the organic kingdoms, moreover, each individual has its historic phases, from the germ through the cycle of growth. The general idea sought out by induction, therefore, is not made up of *in*variables. Limited to these, it represents no object, class of objects, or law, in nature. The variables are a necessary complement to the invariables; and the complete species-idea is present to the mind, only when the image in view is seen to be ever changing along the lines of variables and development. Whatever individualized conception is entertained, it is evidently a conception of the species in one of its phases,—that is, under some one specific condition as to size, form, color, constitution, etc., as regards each part in the structure, from among the many variations in all these respects that are possible: mind can picture to itself individuals only and not species, and one phase at a time in the life of an organic individual, not the whole cycle.

We may attempt to reach what is called the typical form of a species, in order to make this the subject of a conception. But even within the closest range of what may be taken as typical

characters, there are still variables; and moreover, we repeat it, no one form, typical though we consider it, can be a full expression of the species, as long as variables are as much an essential part of its idea as constants. The advantage of fixing upon some one variety as the typical form of a species is this,—that the mind may have an initial term for the laws embraced under the idea of the species, or an assumed center of radiation for its variant series, so as more easily to comprehend those laws.

Again, abrupt transitions and not indefinite shadings have been shown to be the law of nature. In proceeding from special characters to a general species-idea, nature gives us help through her stepping stones and barriers. In former times, man looked at iron and other metals from the outside only, and searching out their differences of sensible characters, gradually eliminated the general notion of each, by the ordinary logical method of generalization. But science now brings the elements to the line and plummet, and reaches a fixed *number* for iron and other elements as to chemical combination, etc. By this means, the studying out of the idea of a species seems almost to have escaped from the domain of logic into that of direct trial by weights and measures. It is no longer the undefined progress of simple reason, with a mere notion at the end, but an appeal to definite measurable values, with stable numbers at bottom, fixed in the very foundations of the universe. So, in the organic kingdoms, where there is, to our limited minds, still greater indefiniteness in most characters, the barrier against hybridity appears to stand as a physical test of species. We are thus enabled in searching into the nature of a species, to strike from the outside detail to the foundation law.

The type-idea, as it presents itself to the mind, is no more a subject of defined conception than any mathematical expression. Could we put in mathematical terms the precise law, in all its comprehensiveness, which is at the basis of the species iron, as we can for one of its qualities, that of chemical attraction, this mathematical expression would stand as a representative of the species; and we might use it in calculations, precisely as we can use any mathematical term. So also, if we could write out in numbers the potential nature of an organic species, or of its germ, including the laws of its variables, this expression would be like any other term in the hands of a mathematician; the mind would receive the formula as an expression for the species, and might compare it with the formulas of other species. But, after all, we have here a mere mathematical abstraction, a symbol for an amount or law of force, which can be turned into conceptions, only by imagining (supposing this possible) the force in the course of its evolution of concrete realities, according to the law of development and laws of variations embraced within it.

ART. XXIII.—*Preliminary Researches on the alleged Influence of Solar Light on the Process of Combustion;* by JOHN LE CONTE, M.D., Professor of Natural Philosophy in the South Carolina College.

[Read before the "American Association for the Advancement of Science," at the Montreal meeting, August, 1857.]

A POPULAR opinion has long prevailed in England, and perhaps in other countries, that the admission of the light of the sun to an ordinary fire tends to *retard* the process of combustion. In some instances, the practice of placing screens before the fireplace, or of closing the shutters of the apartment, may be traced to the prevalent belief, that the access of sunlight to the burning materials is unfavorable to the continuance of the phenomenon of combustion. Most physical philosophers very naturally regard this opinion as a mere *popular prejudice;* probably originating in the well-known apparent dulling or obscuration of flames and of solid bodies in a state of ignition,—which takes place when they are exposed to strong light. The flame of a jet of burning hydrogen is scarcely visible in the diffused light of a clear day; that of an ordinary alcohol lamp is barely appreciable to the eye when exposed to the direct sunshine; while a portion of ignited charcoal which glows in the dark, appears to be extinguished when placed in the sunlight. These familiar phenomena, attributable to well-established physico-physiological laws, seem to afford a much more rational explanation of the origin of the popular opinion, than to suppose it to be based upon observations relating to the actual rapidity of burning.

About thirty-two years ago, Dr. Thomas M'Keever published a series of experiments in the "Annals of Philosophy,* which seemed to show, that there is a real foundation for the popular impression, and that solar light does actually retard the process of combustion. So far as I am aware, these remarkable experiments have never been repeated. Leopold Gmelin, in his "Hand-book of Chemistry,"† announces Dr. M'Keever's results without comment. The important bearing which they appear to have on the influence of solar light on chemical processes, as well as on the modern dynamical theory of the mutual convertibility of the so-called imponderables, induced me during the months of May and June last, to undertake a series of experiments with the view of testing the validity of Dr. M'Keever's

* Annals of Philosophy, New Series, vol. 10, p. 344, Nov. 1825.

† Leopold Gmelin's Hand-book of Chemistry, (Cavendish Society's Translation,) vol. 2, p. 35. London, 1849.—A contemporary journal, in noticing these results, remarks, "It has always been considered a vulgar error, that the sun's light extinguishes a fire, but the following experiments by Dr. M'Keever put the matter beyond a doubt." (Brewster's Edin. Journal of Science, vol. 5, p. 180. 1826.)

conclusions. The subjoined table will exhibit his results in a convenient form for future reference.

Expt. 1.—Green Wax Taper lost in 5 minutes

In Dark, Temp. 67°	In Sunshine, Temp. 78°	Ratio.
9·25 grs.	8·5 grs.	1 : 1·088

Expt. 2.—Taper Lost, by Burning 7 minutes, in

Dark, Temp. 67°	Sunshine, Temp. 78°	Ratio.
11 grs.	10 grs.	1 : 1·100

Expt. 3.—Mould Candle, to consume 1 inch took, in

Dark, Temp. 68°	Sunshine, Temp. 80°	Ratio.
56m 0s	59m 0s	1 : 1·053

Expt. 4.—Taper, to consume 1 inch took, in

Dark, Temp. 67°	Sunshine, Temp. 79°	Ratio.
4m 30s	5m 0s	1 : 1·111

Expt. 5.—Taper in Sunshine lost in 10 minutes

In Painted Lantern.	In Uncoated Lantern.	Ratio.
16·5 grs.	15 grs.	1 : 1·100

A *sixth* experiment, of a similar character, made in a strong *moonlight* indicated no such diminution in the rate of consumption.

The conclusion to which Dr. M'Keever came was, that solar light *does exercise* a positive retarding influence on the process of combustion. He supposes this effect to be owing to the well-known influence of the solar rays on many chemical processes:—in some instances accelerating them, but in others retarding them. Under this point of view, the *chemical rays* may be supposed to exercise a *deoxydizing* power, which, to some extent, interferes with the rapid oxydation of the combustible matter. In confirmation of this opinion, Dr. M'Keever made an experiment which appears to indicate that a taper burns more rapidly in the *red* than in the *violet* extremity of the solar spectrum.

In attempting a repetition of Dr. M'Keever's experiments, I found it impossible to secure that freedom from agitation in the atmosphere, during the exposure of the burning body to the influence of sunshine in the *open air*, which such an investigation demanded. This was his method of conducting the *first four* experiments given in the table. The powerful influence exercised by comparatively slight disturbances in the air, on the rapidity of combustion, renders attention to this circumstance of controlling importance. His method of obviating this difficulty by the use of lanterns, (as indicated in Experiment 5,) is objectionable, from the impossibility of securing precisely identical conditions in relation to the supply of air in the interior. There were, likewise, other considerations which urged me to modify his method of conducting the investigation. It occurred to me,

that, as in his experiments the *temperature* of the air which supplied oxygen for combustion in the sunshine, was about 12° Fah. above that in the darkened room, the *rarefaction* produced by heat might exercise some influence in retarding the rate of burning in the sunlight.

In conducting my experiments, I endeavored to secure *two conditions*, viz:

1. Absolute *calmness* in the atmosphere.

2. Exposure of the flame to the influence of intense solar light, *without heating the surrounding air.*

The *first* condition was secured, by performing *all* of the experiments in a large lecture-room, with all the doors and windows closed. To secure the *second* condition, I employed a portion of the apparatus belonging to a large solar microscope, consisting of the reflecting mirror, the condensing lens and tube, together with the mechanical arrangements for adjusting the direction of the light. As the condensing lens was upwards of four inches in diameter, I hoped to *exaggerate* enormously whatever effect the light might exert, by concentrating it on a comparatively small area. Inasmuch as the aperture in the window through which the light was admitted, was completely closed by this arrangement, the exterior agitations of the atmosphere were not felt in the room; while the pencil of light, thus thrown on the flame, traversed it, as well as the surrounding air, *without imparting a sensible amount of heat to the latter.*

I used the best *wax-candles*, (as they are called in the shops,) *four* to the pound, costing about 15 cents apiece.* By allowing them to burn a sufficient length of time to form a well-defined cup for the melted wax, and carefully turning the wicks so as to render them self-snuffing, the combustion was found to go on with remarkable uniformity in a calm atmosphere. The *rate of burning* was determined in the following manner:—A portion of candle, three or four inches in length, was secured to the bottom of one of the scale-pans of a tall balance and ignited; after allowing it to burn for ten or fifteen minutes, so as to secure a steady flame of constant size, it was *nearly* balanced by adding weights to the opposite scale-pan, allowing a slight preponderance to the candle-pan. In a short time the equilibrium was established by the burning of the candle;—the precise *time* at which the balance indicated a condition of equilibrium was *accurately noted.* Next, *a given* weight, (say 60 or 100 grains,) was withdrawn from the weight-pan, and the *time* of restoring the

* From the close approximation to *identity* in the rate of consumption, it is probable that these are the same as Dr. Ure's "genuine wax-candles." He found the consumption to be, "upon an average of many experiments, 125 grains per hour." (Dict. of Arts, Manuf. and Mines, 4th edition, Article, "Illumination, Cost of,") My experiments gave respectively, 136·7, 125·4, and 124·7 grains per hour. Other kinds of candles burn at a much more rapid rate.

equilibrium by the loss of weight in the burning candle, was, in like manner, recorded. In this manner, the rate of combustion was determined by observing the *time* occupied in consuming a *given weight* of the burning matter. The arrangements described above, enabled me to perform such experiments alternately in the darkened room, and in the concentrated sunbeam, *without moving* any portion of the apparatus in the room, and under external conditions as nearly identical as could be desired. Many preliminary experiments were made for the purpose of testing the delicacy of the arrangements, which very soon convinced me that no reliable results could be obtained unless the air was calm, and *also*, unless the candle was allowed to burn a *sufficient length of time* to establish *regularity* in the process of combustion. The days selected for the experiments, were perfectly *cloudless*. The state of the barometer and thermometer were carefully noted. I now regret that I neglected to record the hygrometric condition of the air. The cone of sunlight was so directed, that its *lower* margin illuminated the charred portion of the wick of the candle, while the *upper* boundary of the pencil traversed the flame near its apex. The following Table presents the results furnished by *three sets* of experiments performed on as many separate days.*

Date.	Bar. reduced to 32° Fah.	Temp. of air, Fah.	Time of consuming 60 grains.		Amount consumed in 10 minutes.		Diff. grs.
			Dark.	Sunlight.	Dark.	Sunlight.	
May 9,	29·92	67°	26m 24s	26m 15s	22·73 grs.	22·86 grs.	−0·13
June 6,	29·72	75°·5	28 39	28 45	20·94 "	20·87 "	+0·07
" 10,	29·62	84°	28 53	28 51	20·75 "	20·80 "	−0·05

It will be observed, that these experiments indicate *no sensible difference* in the rate of combustion of the candle in the darkened room, and in the same apartment, with a pencil of concentrated sunlight directed on the flame; *provided*, the comparison is restricted to the results obtained on *any given day*. In two instances there was a slight *excess* in the rate of burning in the *sunshine*, and in the other, the excess was in favor of the *dark:* but the differences are probably within the limits of the experimental inaccuracies incident to such delicate investigations.

* The difficulty of keeping the pencil of solar light properly directed on a flame of *variable* altitude, induced me to try a "Burning Fluid" lamp having a short cylindrical reservoir, furnished with *two* wicks. The following results were obtained:

Date.	Bar.	Temp.	Amount consumed in 10 minutes.	
June 3d,	29·72	74° F.	Dark = 40·82 grains.	Sun = 40·00 grs.
" 4th,	29·78	75° "	" = 40·95 "	" = 39·48 "
" 4th,	29·78	75° "	" = 38·34 "	
" 4th,	29·78	75° "	" = 37·52 "	

The progressive *decrease* in the rate of combustion, as indicated by the experiments of the 4th of June, shows, that the variations in the *height of the liquid*, are sufficient to vitiate the results obtained from burning any *fluid material*.

These *negative results* are the more striking from the fact, that, if solar light exercised the decided influence on the process of combustion which Dr. M'Keever's experiments seem to indicate, we should expect the effects to be much more marked and conspicuous, when the light was increased in intensity from *eight to ten fold* by the concentration of a lens. The fact that the rays of the sun traversed the *glass lens* before they fell on the flame, can scarcely be urged as a possible explanation of the discrepancy; for Dr. M'Keever obtained analogous results when he employed *lanterns*. (Vide experiment No. 5.)

The obvious variation in the rapidity of combustion on *different days*, (as exhibited in my experiments,) illustrates in a most striking manner, the decided influence exercised on the process, by comparatively slight alterations in the external conditions. This fact should inspire us with wholesome caution, and check the spirit of rash generalization. Throwing out of consideration the possible fluctuations in the rate of burning arising from want of homogeneity in the combustible materials, and imperfections in the mechanical arrangements by which they are consumed, there are *three external conditions* which may be supposed to exercise more or less influence on the rapidity of the process. These are, *first*, barometric pressure; *second*, temperature of the air; and *third*, amount of aqueous vapor present. I propose to consider each of these separately.

1. *Barometric Pressure.*

From *à priori* considerations, we should be led to expect, that an increase of barometric pressure, through the consequent condensation of the air, would, *cæteris paribus*, tend to *augment* the rapidity of combustion, by furnishing the burning matter with a greater amount of oxygen in a given volume. Unfortunately, direct experiments are wanting to test this in as satisfactory a manner as we should desire. The older experiments in the Boylean vacuum, inasmuch as they relate to the degree of rarefaction at which combustion *ceased*, do not give us information in regard to the *rapidity* of the process at the various stages of exhaustion. The same remark applies to the later experiments of Grotthuss, as well as to the admirable "Researches" of Sir Humphry Davy on the "Effects of Rarefaction, by partly removing the Pressure of the atmosphere, upon Flame and Explosion."* The experiments of the latter show, that rarefaction produces striking alterations in the size and character of the flame, but do not touch the question of the relative *rate* of burning under different pressures:—they test the comparative *combustibility* of dif-

* Vide Davy's "Researches on Flame," Phil. Trans. for 1817, p. 45 et seq. Also, Works of Sir H. Davy, edited by Dr. John Davy. Vol. 6, p. 51 et seq. Lond. 1840.

ferent bodies, rather than the rapidity of consumption of a *given body* under various degrees of rarefaction. Nevertheless, Davy informs us, that he determined from actual experiment, that the amount of *heat* developed in a *given time* by combustion, is slowly *diminished* by rarefaction; "the diminution of the cooling power of the nitrogen, being apparently in a higher ratio than the diminution of the heating powers of the burning bodies." Speaking of the phenomena of combustion in *condensed* air, he says, "I ascertained, however, that both the light and *heat* of the flames of the taper, of sulphur and hydrogen, were increased by acting on them by air condensed four times; but not more than they would have been by an addition of $\frac{1}{5}$ of oxygen." Again, he says, "But by compression, there can be no doubt, the heat of flames from *pure* supporters and combustible matter may be greatly increased, probably in the ratio of their compression:"—in the case of air he does not think the effect would be so great. Inasmuch as the quantity of heat developed in a *given time* by the burning of a *given substance*, is known to be a measure of the amount of matter undergoing oxydation, we are justified in the inference, that the foregoing results of Sir H. Davy's experiments, show that the *rate* of combustion was retarded by the rarefaction and accelerated by the condensation of the air.

The most satisfactory results in relation to the influence of *condensed air* on the process of combustion, are those incidentally furnished about sixteen years ago, by M. Triger, a French civil engineer, during the operations necessary for working a bed of coal lying under the alluvium bordering the river Loire, near Languin in the department of *Maine-et-Loire*. In traversing an overlying stratum of quicksand from 59 to 65½ feet thick, he found it requisite to devise some means of excluding the semi-fluid quicksand and water, which found their way under every arrangement analogous to ordinary *cofferdams*, in such quantity as to defy all pumping operations intended to keep them dry. For this purpose, M. Triger employed large sheet-iron cylinders, about 3·39 feet in interior diameter, securely closed at the top, in which,—by means of a condensing pump incessantly worked by a steam-engine,—air was condensed to an amount sufficient to counteract the external hydrostatic pressure. The ingenious contrivance fully justified the expectations of the engineer; but the workmen were thus compelled to labor in air condensed under a pressure of about *three* atmospheres. Among other curious results of this state of things noticed by M. Triger, were the remarkable effects of condensed air on combustion. Much annoyance was at first experienced from the *rapid combustion* of the candles; which was only obviated by substituting flax for cotton threads in the wicks.* Similar phenomena were observed a few

* *Vide*, Comptes Rendus, Tome 13, p. 884, et seq., Paris, 1841. Also, Annales de Chimie et de Physique, 3d Series, Tome 3, p. 234, et seq., Paris, 1841.—The fol-

years ago, by the engineers of the Wilmington and Manchester railway, who employed analogous apparatus for securing the foundations for the piers of the railroad bridge across the Great Pee Dee river in South Carolina. So far as I have been able to ascertain, the results manifested in this case, were identical with those recorded by M. Triger, and afford a most striking confirmation of the influence of condensed air in *accelerating* the process of combustion.

On the other hand, facts are not wanting to prove, that combustion is *retarded* at considerable elevations above the ocean, where the air is rarefied by diminished pressure. In a letter recently communicated to the Royal Society of London, from J. Mitchell, Esq., Quartermaster of artillery at Bangalore, India, "On the Influence of Local Altitude on the Burning of the Fuses of Shells;" this officer shows, that there was a *progressive retardation* of the rate of combustion of the fuses, at altitudes of 3000, 6500, and 7300 feet, as contrasted with the rapidity of burning at the Artillery Depot yard. This difference, Mr. Mitchell, very rationally, attributes, "to the rarity of the atmospheric air, and of its constituent oxygen, at the higher stations."* The following table,—in which I have reduced the barometric heights to the *freezing point*,—exhibits the *mean results* of his experiments.

Height in Feet.	Bar. at 32° F.	Temp. Fahr.	Average Time of Burning 3 inches of Fuse.	No. of Expts.
Depôt.	29·610	89°	14·25 seconds.	6
3000 feet.	26·755	82°	15·78 "	5
6500 "	23·951	61°·8	17·10 "	3
7300 "	22·979	54°·2	18·125 "	2

These experiments seem to have been made with great care; all in the presence of artillery officers, who were furnished with the most accurate methods of measuring time. They amply prove the fact, that combustion is retarded at considerable elevations.

Thus, a variety of well-established facts, concur in fortifying the conclusions to which we are led by *à priori* reasoning; *namely*, that the process of combustion is *retarded* by diminution of the density of the air, while it is *accelerated* by its condensation. It has long been a matter of common observation, that ordinary wood-fires burn more freely when the barometer is high; but, Mr. Marcus Bull and others maintain,† that this result is not

lowing are the words of M. Triger, "A la pression de trois atmosphères, cette accélération devient telle que nous avons été obligés de renoncer aux chandelles à mèches de coton pour les remplacer par des chandelles à mèches de fil. Les premiéres brûlaient avec une telle rapidité, qu'elles duraient à peine un quart d'heure, et elles rèpandaient en outre une fumée intolérable."

* Philosophical Magazine, 4th Series, vol. 10, p. 48, July, 1855. Fuses burn *without air*; but the *rate* of burning is influenced by atmospheric oxygen.

† *Vide*, Trans. of Am. Philosophical Society, 2d Series, vol. 3, p. 55-56.—Philadelphia, 1830.

owing to the augmented *density* of the air, but to the greater *dryness* of the atmosphere. The facts brought forward in this paper, are strongly opposed to this explanation; for, there are not the slighest grounds for supposing, that there was *less* than the ordinary amount of aqueous vapor present in the condensing cylinders of M. Triger; or *more* than the usual quantity mixed with the air at the elevated stations in India. On the contrary, physical considerations lead us to precisely *opposite* conclusions.

2. *Temperature of the Air.*

In relation to the influence of the *temperature* of the air on the *rate* of combustion, our information is still more meagre. The experiments of Grotthuss and Sir H. Davy on the "Effects of Rarefaction by Heat on Combustion and Explosion," give contradictory results:* but, as they relate exclusively to the influence of temperature on the *ignition* of explosive mixtures of gases, they test its effects on *combustibility*, and are obviously inapplicable to the question under consideration. The well-known effects of the "Hot Blast" in increasing the temperature of furnaces, cannot be applied as a test of the influence of warm air on the rate of combustion under ordinary circumstances. *First*, because the air of the "hot blast" is *not* in its natural state of *density;* and *secondly*, because the augmentation of temperature observed in such cases, probably arises from its greater *availability*, growing out of the fact, that less heat is carried off in the products of combustion, rather than an absolute increase in the *rapidity* of burning.

In the absence of direct experimental evidence, it may be admissible to apply general reasoning based upon well-known physical principles. So far as an increase of temperature influences the *density* of the air, it is sufficiently evident, that its effect must be equivalent to a diminution of barometric pressure; and, consequently, must tend to *retard* the process of combustion. Assuming the temperature of the flame to be *constant*, it is likewise plain, that the *draught* created by it,—depending, as it is known, on the *difference* of temperature between the flame and that of the surrounding air,—must be *diminished* in a warm atmosphere; and, therefore, also tend to *retard* the rate of combustion, *cæteris paribus*, during hot seasons. But inasmuch as the variations in the velocity of the draught, are proportional to the *square roots* of these differences of temperature, it is obvious, that its effects must be insignificant under ordinary fluctuations of atmospheric temperature. For example, supposing the temperature of the flame to be 1500° Fahr., then the fluctuation of the draught between the temperatures of 80° and 60° Fahr., would

* Phil. Trans., for 1817, p. 53.

be in the ratio of $\sqrt{1500-80} : \sqrt{1500-60} = \sqrt{1420} : \sqrt{1440} = 1 : 1{\cdot}0070$. When, however, the comparison is made between bodies burning in summer and in winter, the influence from this cause will be more sensible, and ought not to be entirely overlooked.*

On the contrary, it is *possible*, that an augmentation of temperature might tend to *accelerate* the process of combustion, by favoring the liquefaction of the wax, and, perhaps, facilitating the oxydation of the combustible matter. If any such influence is exercised, it is probable, however, that its effect must be inappreciable under ordinary circumstances. Under this view of the subject, the only obvious influence which atmospheric heat exercises on the rapidity of combustion, is connected with its effects on the *density* of the air: and that, consequently, an increase of temperature should, *cæteris paribus*, *retard* combustion, and *vice versa*.

3. *Amount of Aqueous Vapor present.*

Sir Humphry Davy found that "a very large quantity" of *steam* was required to prevent sulphur from burning; that an explosive mixture of oxygen and hydrogen, when mixed with 5 *times its volume of steam*, still exploded by the electric spark; and that a mixture of air and carburetted hydrogen gas, required "a *third* of steam to prevent its explosion, whereas $\frac{1}{5}$ of azote produced the effect."† Under any point of view, it is obvious, that the presence of aqueous vapor can only tend to *retard* the process of combustion. *First*, because it diminishes the amount of oxygen in a given volume of air, and *secondly*, because an admixture of any inactive gas tends to extinguish the burning body, as is abundantly proved by the experiments of Sir H. Davy and others. When vapor is present in large quantities, there can be no doubt of its controlling agency on combustion. This is illustrated by the successful application of the plan proposed by M. Dujardin of Lille, in 1837, for extinguishing fires occurring in steam-ships, by permitting the steam from the boilers to escape into the apartment in which the combustion originates.‡ But experiments are still wanting, for determining its influence on the rate of burning, when existing in the small quantities in which it is usually associated with the atmosphere.§ The exper-

* I endeavored to test the influence of *temperature* on the rate of combustion, by placing the burning candle over a *large heated plate*; but,—as might have been expected,—the *unsteadiness of the flame* rendered the experiment unsatisfactory.

† Phil. Trans. for 1817, p. 65.

‡ Comptes Rendus, Tome 5, p. 28. Paris, 1837: also, Tome 35, p. 368, et 706. Paris, 1852.

§ The curious results obtained by Mr. J. F. Dana, and subsequently Mr. Samuel Morey, in relation to increasing the *brightness* of the flames of highly carbonaceous combustibles, by throwing a jet of steam into them, are obviously inapplicable to candles. (*Vide*, Silliman's Journal, 1st Series, vol. 1, p. 401; vol. 2, p. 118, 122; and vol. 7, p. 141.)

imental researches of Mr. David Waldie, in relation to the mixture of various gases with air, led him to the *general law*, that, "Of incombustible gases which remain undecomposed, the power of preventing combustion is in the order of their density:" and that, "This effect of density in cooling the flame depends on the excessive *diffusion* of the flame in the denser gas."* Under ordinary circumstances, the *density* of the aqueous vapor existing in the air is comparatively *small*, so that, according to Mr. Waldie's law, its influence on combustion ought not to be very striking. It is very desirable that this point should be submitted to a more rigorous experimental investigation.

Having discussed the probable influence of the *three external conditions* on the rate of combustion, we are, in a measure, prepared to investigate their *adequacy* to explain the variations in the *rapidity of burning*, as indicated by the experiments which I have brought forward. In *none* of them, have we the observations necessary for ascertaining the *hygrometric condition* of the atmosphere: this must, therefore, be thrown out of consideration. In Dr. M'Keever's experiments, the *barometric* indications are *not* given; neither is it known how many of them were performed on any *one day*. In my experiments, as well as in those of Mr. Mitchell, we are furnished with the data requisite for estimating the *combined* influence of *pressure and temperature*. Assuming, with Sir H. Davy, that the *rapidity* of combustion is in the *direct ratio of the density of the air*, we may submit these two effects to a *quantitative* estimation, by using Mariotte's law and Regnault's coëfficient of expansion for air.

1. Presuming that *each set* of experiments made by Dr. M'Keever, alternately in the dark and in the sunshine, were performed on the *same day*, and therefore under *identical* barometric conditions, we may form some estimate of the adequacy of *temperature* to account for the difference in the rate of burning observed by him. The subjoined table, which I have constructed from the data previously given, will place this in a clear light.

Expt.	Temp. Dark.	Temp. Sun.	Consumed in 10 minutes, in Dark.	Consumed in 10 minutes, in Sun.	Ratio of Amt. in Sun & Dark.	Ratio of *Den.* Air in Sun & Dark.
No. 1.	67° F.	78° F.	18·50 grs.	17·00 grs.	1 : 1·088	1 : 1·021
No. 2,	67° "	78° "	15·72 "	14·29 "	1 : 1·100	1 : 1·021
No. 3,	68° "	80° "			1 : 1·053	1 : 1·023
No. 4,	67° "	79° "			1 : 1·111	1 : 1·023

The remarkable discrepancies indicated by the numbers in the first column of *ratios*, afford a striking illustration of the existence of some disturbing cause, tending to vitiate the accuracy of these experiments. But a glance at the numbers contained in the two columns of *ratios*, is sufficient to show, that *temperature alone* is entirely *inadequate* to account for the diminished rate

* *Vide*, Philosophical Magazine, 3d Series, vol. 13, p. 86, et seq. August, 1838.

of combustion in the sunshine. A remarkable difference is observed in the *rate* of consumption in experiments 1 and 2. Number 1 was made with a "green wax taper," and No. 2 with a "taper;" but, as from the context, the *second* experiment appears to be a repetition of the *first*, the presumption is, that the same kind of taper was used in both cases. The rate of burning in experiments Nos. 3 and 4, was determined by the *time* required to consume a *given length;* and as one of them was made with a mould-candle and the other with a taper, no comparison can be extended to them, so far as the rates of consumption in these two cases are concerned. The irregularities exhibited in these results, probably arose from the agitations of the atmosphere, which were incident to the method of exposing the burning body to the sunshine in the *open air*. As the *excess* of the consumption in the dark, varied from 5 to 11 per cent, whereas the excess in the density of the air was only 2·3 per cent, it is evident, that some *other* cause than *temperature* must be evoked to explain the difference.

2. In my experiments, the conditions were such as to eliminate the effects of *temperature* on the results obtained in the dark and in the sunshine, on any *given day;* and it has been shown, that for *each pair* of experiments thus conducted, the variations in rate of combustion *do not* exceed the probable limits of experimental error. In this case, therefore, the question to be determined is, whether the differences in the *rapidity of burning* observed on *different days*, can be explained by the variations of the barometer and thermometer? For this purpose, I shall take the *average* of each *pair* of experiments, as a nearer approximation to the correct rate of burning on each of the three days. The following table, in which the *relative densities* of the air have been calculated, by *combining* the effects of barometric and thermometric oscillations, will serve to illustrate this point.*

Ratio of Consumption.	Ratio of Density of Air.	Diff.
In exp't 1 & 2=1 : 1·0904	1 : 1·0230	+6·74 per cent.
" " 2 & 3=1 : 1·0063	1 : 1·0193	−1·30 "
" " 1 & 3=1 : 1·0967	1 : 1·0428	+5·39 "

It will be seen, that the rate of combustion increases in a decidedly *higher ratio* than the *density* of the air. If, therefore, we assume that the rapidity of burning is, *cæteris paribus*, in the direct ratio of the density of the air, it follows, that some *other agency* must have coöperated in these cases.

* In making these calculations, I used the following formula, based upon the two well-known physical laws, that the *density* of any permanent gas, varies *directly* as the compressing force, and *inversely* as the volume:

$$d : d' :: \frac{b}{1+0{\cdot}002036(t-32)} : \frac{b'}{1+0{\cdot}002036(t'-32)};$$

in which d and d' represent the *densities;* b and b' the *barometric heights* reduced to the freezing point; and t and t' the *temperatures* on Fahrenheit's scale.

3. The results of Mr. Mitchell's experiments at different altitudes, may, in like manner, be subjected to a similar numerical test. The subjoined table is constructed from the data contained in that which is given on a previous page.

Ratio of Rate of Burning.	Ratio of Density of Air.	Diff.
In exp'ts 1 & 2 = 1 : 1·1074	1 : 1·0926	+1·48 per cent.
" " 2 & 3 = 1 : 1·0836	1 : 1·0755	+0·81 "
" " 3 & 4 = 1 : 1·0599	1 : 1·0270	+3·29 "
" " 1 & 3 = 1 : 1·2000	1 : 1·1751	+2·49 "
" " 2 & 4 = 1 : 1·1486	1 : 1·1045	+4·41 "
" " 1 & 4 = 1 : 1·2719	1 : 1·2068	+6·51 "

This comparison places in a still stronger light the fact that the augmentation in the rate of burning, increases in a somewhat *higher ratio than the density of the air;* while at the same time, it clearly demonstrates the *controlling influence of atmospheric density* on the phenomenon of combustion. The *extreme rates* of burning are as the numbers 100 to 127, while the corresponding *densities* of the air are as 100 to 121, nearly; in the other cases the approximation to *identity* in the ratios is still closer. Would the variations in the hygrometric state of the atmosphere, which we have left out of consideration, explain this discrepancy? In the absence of the experiments necessary for testing this question, it would be premature to hazard any conjecture. I may remark, however, that in the case of Mr. Mitchell's experiments, the correction for the effects of aqueous vapor would, probably, *in one point of view*, operate in the *wrong direction*, and thus tend to increase the discrepancy in the ratios. For as the temperature was decidedly *higher* at the lower stations, it is more than probable that the *tension of vapor* was *greater* there than at the upper ones; and, consequently, that its influence in retarding combustion should be *relatively greater* at the points nearer the sea-level. This, of course, would tend to *equalize* the rates of burning at lower and higher altitudes, when no correction is made for this cause. On the contrary, it is obvious that the influence of vapor having a *given tension* in altering the relative amount of air in a *given volume*, must be *greater* when the barometer is low. From this cause the aqueous vapor at the upper stations might have had a *greater* effect in retarding combustion, and thus tended to *exaggerate* the difference in the rates of burning.

The comparatively large rate of consumption indicated by my *first* experiment of the 9th of May, (being more than nine per cent above the others,) was most probably attributable to a combination of causes. All of the *three external conditions* concurred in accelerating the process. The barometer was *high*, the temperature *low*, and the atmosphere *excessively dry*. The last-mentioned condition was accidentally forced upon my attention from

the fact that on that day I failed in an experiment for determining the dew-point by means of Daniell's hygrometer.*

From the foregoing discussion it is evident that the subject demands a thorough experimental investigation, with a minute at-

* Collaterally related to this subject are the effects of condensed and rarefied air and of temperature on the *process of respiration and the elimination of carbonic acid* in men and other warm-blooded animals. M. Legallois found that when warm-blooded animals breathed air under *pressure* reduced to 11·811 inches, the amount of oxygen consumed was *diminished.* (Ann. de Chemie et de Phys., tome 4, p. 113, 1817.) M. Théodore Junod's experiments show that *condensed air* produced deep inspirations and an agreeable glow throughout the system; while *rarefied air* had an opposite effect. (Archives Générales de Médecine, 2d series, tome 9, p. 157. Paris, 1835. Also, Magendie's Report on the same Memoir, Comptes Rendus, tome 1, p. 60. Paris, 1835.) The observations of M. Triger, already referred to, indicate analogous effects on those who labored in the condensed air. They could do double work without fatigue, and even old asthmatics seemed to recover their vigor. (Comptes Rendus, tome 13, p. 884, et seq. Paris, 1841.) M. Vierordt tested the effects of barometric pressure between 29·309 and 30·197 inches. The *average* rise of 0·5036 of an inch,

Increased the air expired				35·746	cub. inches	per minute.
"	" No. of respirations			0·74	"	"
"	" " " pulse			1·30	"	"

Dr. Hutchinson found that in a mine 1488 feet deep, where the pressure was 1·54 inches more than at the sea-level, the respiration was *increased* 2·4 per minute, and the pulse 1·3 per minute.(Cyc. of Anat. et Physiol. [article Respiration], vol. 4, p. 348–349. Lond., 1852.)

Analogous effects are produced by *temperature.* In the famous experiments of Séguin and Lavoisier, at 82° Fahr., the former (fasting and at rest) consumed 1210 French cubic inches of oxygen per hour; whereas at 57°, he consumed 1344 cubic inches per hour. (Mémoires de l'Acad. Royale, for 1789.) Dr. Crawford found, that a Guinea pig at 55°·5 F. abstracted *twice* as much oxygen from the air as at 104° F. (Exp'ts and Obs. on Animal Heat, 2d ed., p. 311–315. Lond., 1788.) Dr. W. F. Edwards found that birds consume more oxygen in winter than in summer. (De l'Influence des Agens Physiques sur la Vie, chap. 6, p. 195. Paris, 1824.) The best experiments are those of M. Vierordt. (*Op. cit. supra.*) He obtained the following results between 37°·4 and 75°·2 Fahr.

	Av. Temp. 47°·24.	Av. Temp. 66°·92.	Diff.
Pulse per minute,	72·93	71·29	1·64
Respiration per minute,	12·16	11·57	0·59
Vol. of Air exp'd per minute in cub. inches,	407·00	367·00	40·00
" " CO_2 " " " " " "	18·25	15·72	2·53

M. Felix Letellier's experiments on warm-blooded animals confirm these results. He found the amount of *carbonic acid* evolved *per hour* at different temperatures, to be as follows:

	86° to 104° F.	59° to 68° F.	32° F.
Canary,	0·129 grammes.	0·250 grammes.	0·325 grammes.
Pigeon,	0·366 "	0·684 "	0·974 "
Two Mice,	0·268 "	0·498 "	0·531 "
Guinea Pig,	1·453 "	2·080 "	3·006 "

(*Vide* Comptes Rendus, tome 20, p. 795. Paris, 1845. Also, Ann. de Chemie et de Phys., 3d series, tome 13, p. 478. Paris, 1845.) Doubtless *physiological reactions* exercise a powerful influence over the results of such experiments; nevertheless, as respiration is *essentially* a process of combustion, they have a *general bearing* on the question under consideration.

tention to all of the external conditions which may influence the results. This I propose to undertake during the next twelve months. In the mean time it is hoped that these preliminary researches may prepare the way for a clearer appreciation of the difficulties which are to be encountered. Perhaps, however, in the present stage, we may be warranted in deducing *two* conclusions: 1st, that *solar light does not* seem to exercise any sensible influence on the process of combustion; and 2dly, that variations in the *density of the air do exert* a striking effect in retarding or accelerating the rapidity of the process, the rate of burning augmenting with every increment of density and *vice versa;* but the exact ratio between them remains to be determined.

Columbia, S. C., June, 1857.

ART. XXIV.—*Researches upon the Influence of Sulphur upon Iron and that of Phosphorus in partially neutralizing the action of the Sulphur;* by M. JANOYER.

(Translated from the Ann. des Mines, 6th vol., for 1854, p. 149, for the Amer. Jour. of Science and Arts, by W. J. TAYLOR.)

THE deleterious influence of sulphur upon the qualities of the various kinds of iron is a fact known to all persons engaged in metallurgy. Indeed, it is well understood that there is a very great difference between the irons manufactured with charcoal and those with mineral coal. These last are nearly all more or less "hot-short," although made from the purest ores. This bad quality of the iron is to be attributed generally to the presence of the sulphur in the mineral coal. Traces of sulphur that can scarcely be detected by analysis, are sufficient to render the iron "hot-short." Such irons, treated in the cold, are soft and tenacious; they are generally wanting in lustre in the fracture, and are welded with great difficulty; on the contrary, they are very brittle when hot, especially at a cherry-red heat, from which fact they have received the name of "fers de couleur."

According to Karsten (Manuel de Metallurgie), 0·03375 per cent of sulphur is sufficient to destroy the property of welding in the iron and to render it completely "hot-short." The latest analyses of the hot-short irons afford only one part of sulphur in 10,000 parts of iron.

The experiments made on this subject by this able metallurgist were carried out with great care. I will mention one operation on a great scale, which sustains his statement, that the process by which the iron receives the sulphur and which therefore is especially to be avoided, is the fusion of the ore in contact with the combustible mineral in the high furnace.

During the year 1850, I treated in the high furnace of L'Orme (Loire) only the ore from Privas (Ardèche), of which I have given the analysis (Annales des Mines, t. xx, 1855). This ore is a hematite (anhydrous sesquioxyd of iron), with shining lustre, unctuous to the touch, leaving upon the fingers a greasy coating of a red color. It is very pure, and of course well adapted to make iron of excellent quality. As evidence, it will be sufficient to consult the excellent memoir by M. Gruner, Ingenieur in chef, des Mines, "On the ores of Privas and Lavoutte," (Ann. des Mines, 3e Serie). From pages 374 and 375 of this memoir the following analyses are taken, proving the purity of the Privas ores.

	Rich ores.		Average ores.
Sesquioxyd of iron,	83·9		60·2
Alumina,	0·8		—
Carbonate of lime,	7·4	Carb. lime, Mag., &c.	29·6
Silica and clay,	6·5		7·0
Water,	1·2	Water and bitum.	3·2
Oxyd of manganese,	trace		—
	99·8		100·0

According to the author, this ore contains scarcely a perceptible trace of sulphur and phosphorus. On page 376 he says, "Sulphur has in vain been sought for, and only the slightest trace of phosphorus has been detected. I ought therefore, from the purity of the ores, to obtain an iron superior to any made in the neighborhood. Notwithstanding this, the iron afforded was moderately "hot-short." It presented, when cold, all the qualities of the best iron; soft, with black fibres and without grain; it could be bent and twisted in every way without producing a flaw. On the contrary, at a cherry-red heat it was very brittle and had lost all its tenacity.

The gangue of the ore was then assayed to determine if it contained barytes, for it is known often to occur with this kind of ore. * * * The experiments proved that sulphate of baryta did not exist in the gangue. The following are the results of two complete analyses of the ore in which all these researches for sulphur were made:

	Ore (Agatisé).	Rich and laminated.
Volatile matters,	1·4	6·4
Silica,	11·0	12·4
Sesquioxyd of iron,	82·0	74·0
Alumina,	1·6	1·0
Lime,	3·6	6·0
Manganese, Sulphur,	none	none

It will be seen from the above that the "hot-short" property of the iron was not owing to the sulphur contained in the ore. It can therefore result only from the sulphids contained in the coke with which it was fused. The coke used in these experiments was invariably that of Peronnière, near Rive-de-Gier

(Loire), the analyses of which (Annales des Mines, 1851, t. xx), afforded 0·28 per cent of sulphur.

The combustible material consumed at this time amounted to 22,000 kilograms (57,200 lbs.) every twenty-four hours; introducing into the furnace in this space of time 61k·600 of sulphur (160 lbs.), which, according to a preceding memoir, is only able to go from the high furnaces as a proto-sulphid in the iron, or in the state of sulphide of calcium in the slags. These last being slightly silicious, the cast iron contained the principal portion of the sulphur.

It was therefore by no means impossible that this quantity should exert, in the high furnace, a very pernicious influence on the product, the cast iron obtained containing 0·003 per cent of sulphur (the yield being 20,000 kilogrammes every twenty-four hours, and supposing, which was not the case, that all the sulphur passed into the cast iron). This protosulphid of iron formed in the metal, should be found from its stability in the wrought iron produced, so that there will then be more than sufficient sulphur to render the iron very "hot-short."

The objection may be made that the sulphur might have been introduced during the transformation of the cast into wrought iron during the puddling in the presence of coal containing much pyrites, which permits a disengagement of half its sulphur at a red heat.

But I say still that the passage of sulphur into the iron certainly takes place in the fusion of the ore in contact with the coke in the furnace stack. For at this time the same ore from Privas was treated in the same high furnace with charcoal, and under the same very careful conditions which were used in fusing the same with coke. The slags were entirely siliceous, like those obtained in the experiments with the mineral coal, which would favor the complete passage of the sulphur into the cast iron by preventing it from escaping as a sulphid of calcium, in the slags. The slag contained Silica 53·77, alumina 17·93,* lime 28·30=100. * * *

The lime was found to be entirely taken up by the silica and consequently could have no affinity for the sulphur. Notwithstanding, the cast iron obtained, puddled in the same furnaces with sand and coal,† by the same workmen, and under exactly the same conditions, gave an iron which presented in all points the characters of the best iron; its tenacity was perfect at all temperatures, presenting none of the characters of a brittle iron at a cherry-red heat.

* The protoxyd of iron, owing to its small quantity, was not separated from the alumina.

† The coal employed came from the mine Du Bois d'Aveize, concession de Terre Noire. It is classified and described by M. Gruner in his memoir (Annales des Mines, t. II, 5e série, p. 166).

Independent of the influence of the sulphur in coke, this experiment shows that it is important to demonstrate a fact practically: the action of sulphur, as will be clearly perceived, was in the fusion of the ore in contact with the combustible mineral, and *not* in the transformation of the cast iron into wrought iron by puddling.

It may hence be concluded that the sulphurous acid disengaged from the coal upon the grate of the puddling furnace and in reheating has no sensible effect on the iron. The sulphid which may be formed in the transformation of the cast iron into wrought iron at the surface of the metallic bath, is always where it is exposed to oxydation, and as M. Berthier has said, it combines with the oxyd of iron, to form oxysulphids, which separate from the iron in the state of scoria and scales. I have never assayed the scoria from the refining without obtaining considerable quantities of sulphur. A careful analysis of a scoria from the refining that was very compact and contained no free iron, gave me 0·152 per cent of sulphur.

These facts established, I sought to prevent this action of sulphur in the high furnace, at least in part, by combining the observations of the learned Karsten (according to whom the best irons appear to contain at least 0·002 to 0·003 per cent of phosphorus) with the observations that I have made upon strong and hard irons made with coke mixed with very pure argillaceous and phosphatic ores. These last mentioned irons possessing none of the properties of the "fers de couleurs," are produced under the same circumstances as the "hot-short" iron which I obtained when the ores contained not the least sulphur, and the coke employed for their fusion was equally sulphurous.

The very soft iron "hot-short" resisted fracture when cold; the two extremities of the bar were brought to within $0^{m}{\cdot}005$ ($\frac{2}{100}$ of an inch) of each other without the least flaw or crack being distinguished in the bend, when the very hard iron has broken when the two extremities were within $5\frac{1}{2}$ inches of each other, (the two pieces of iron used in these trials were of the same length). On the contrary, when the very soft iron, heated at a "cherry-red" was bent to two points, say A and B, it broke without presenting the least tenacity; while the very hard iron perfectly resisted the trial when heated at the same temperature.

The very hard iron, although less good when cold than the very soft iron, generally considered was preferable to the "hot-short" iron, the working of it heated being easier. These irons, as has already been said, were produced under the same circumstances, excepting the mixture of phosphatic ores for the very hard iron.

Persuaded then that phosphorus here plays an important part, I made some trials on a large scale in the high furnace by intro-

ducing phosphorus into the cast iron. I took for this purpose the ore of Villebois (Ain), an oolitic mineral very phosphatic, which, according to the analyses of M. Berthier (Essai par la voie sèche, t. II, p. 231), contains, Sesquioxyd of iron 34·8, water 12·6, phosphoric acid 0·2, clay 34·4, carbonate of lime 18·0. Owing to the large proportion of phosphorus, I added this ore in very small quantities so as not to obtain an iron "cold-short." Experiment determined that the proportion should be $\frac{1}{5}$ of the charge of ore.

I had then a bed of fusion equal to 240 kilogrammes (624 lbs.) of cast metal containing $0^{k}{\cdot}106$ ($\frac{3}{1000}$ of a lb.) of phosphorus, which gives an iron containing 0·00045 per cent of this element. I obtained by this means irons far superior to those previously obtained, and without any loss of its tenacity in the cold; they did not contain the least trace of the "hot-short" qualities. I will cite an example: A piece of iron $0^{m}{\cdot}055$ by $0^{m}{\cdot}015$ ($2\times\frac{1}{2}$ in.), manufactured without admixture of phosphatic ore, when heated, broke at every bend; and a piece of the same size manufactured from the cast metal, containing 0·00045 per cent of phosphorus, resisted fracture perfectly.

The explanation of this remarkable fact already recognized in practical metallurgy is thence apparent; the iron and the castings are much better as the qualities and varieties of ores mixed in the bed of fusion are more numerous, and the amelioration of quality is owing to the influence of foreign bodies, such as manganese and phosphorus, from the different ores.*

We cannot disregard, in this case, the influence of phosphorus, which was the only agent introduced into the new portion. The constitution of the slag was the same, and the cast metal had been produced under the same circumstances; the puddling and the reheating were made in the same manner and with the same fuel.

But how is it that the phosphorus can counteract the influence of the sulphur? This question I shall endeavor to answer by deduction from the facts and experiments made by me during the last two years, persuaded that it is of interest for science, since it throws light on the triple combinations of carbon, iron and phosphorus, besides being of great use in practice.

To arrive at a knowledge of this action of phosphorus upon sulphur in the cast metal, I made two experiments as follows. I remelted in a naked crucible in a forge: 1st, $3\frac{1}{2}$ gram. of an excellent gray white iron, containing much graphite with 0·14 pyrites (bisulphuret of iron). 2d, $3\frac{1}{2}$ gram. of the same iron

* *Note by W. J. T.*—According to Schafhäutl (J. fur Chem. xl, 304,) cast iron, bar iron and steel, almost always contain more or less arsenic and phosphorus. Thus the celebrated Dannemora iron and the English Low-Moor iron owe their good qualities to the presence of arsenic; and a particular kind of Russian iron (marked CC ND) from Demidoff's works at Nischnitagilsk is indebted for its peculiar properties to the phosphorus which it contains. (Gmelin, (Cavendish edit.), vol. v, p. 214.)

with 0·14 pyrites, 0·14 calcined bones, and 0·09 of a refractory white clay.

In these two trials I introduced into the cast iron a fixed quantity of sulphur: but in the last I added also a certain quantity of phosphorus; for the predisposing affinity of the silica for the bases at a high temperature would necessarily lead to the decomposition of the phosphate of lime of the calcined bones, and the phosphoric acid reduced by the carbon of the cast iron would combine with the iron to form a phosphuret of iron. The experiment succeeded perfectly. The fusion was complete and the temperature was pushed even to the softening of the crucible. The two buttons when broken were entirely white, as I have mentioned in a previous memoir, "*Sur l'influence de soufre sur la nature des fontes.*"

The button which contained only the sulphur, bent quite well under the hammer although the cast iron was of a very bad quality; it was only a mass of filamentary crystallizations of protosulphid of iron. This cast iron had a dull aspect, and it was very difficult to pulverize in a mortar; it flattened so as to afford small plates. Certain portions especially were very ductile. These portions doubtless contained some of the iron free: for it is known according to the researches of M. Fournet, "*Sur les sulfures metalliques,*" contrary to the assertions of Karsten (Manuel de Metallurgie, p. 122), that the proto-sulphid of iron can be partially decomposed in presence of carbon at a high temperature. This learned geologist in heating to 150° (pyrometer) in a brasqued crucible some pyrites, obtained a button of protosulphid of iron which was scarcely at all magnetic. This same button, heated strongly a second time under the same circumstances, became after this second fusion strongly magnetic, from which fact it is concluded that a loss of sulphur had resulted, and a production of free iron in the proto-sulphid.

The button which contained some sulphur and phosphorus was, unlike the first, very brittle; it is easily pulverized and does not yield in the least under the hammer.

From this frequently exemplified fact in practice, the conclusion has been deduced: that phosphorus injures to a great degree the tenacity of cast iron. In the second button there could be distinguished by means of a lens parts of sulphurous crystallizations and some crystalline particles showing very brilliant facets.

The buttons from the two assays were pulverized and analyzed. * * * The following are the result of two analyses.

The experiment made with pyrites alone gave with 1·77 gram.

Sulphur not attacked by acids, - - -	0·0150 gram.
Sulphate of baryta 0·33, sulphur, - - -	·0455 "
	0·0605=1·714 p. c.

The fusion made with pyrites and burnt bones gave with 1·77 gram.

Sulphur not attacked by acids, - - - -	0·0200 gram.
Sulphate of baryta 0·23, sulphur, - - -	0·0320 "
	·0520=1·486 p. c.
Loss of sulphur, - - - - - - - -	0·228 "

Another gray iron fused with pyrites alone and again with pyrites and burnt bones gave by analysis of two grammes the following results:

Fusion with pyrites alone. Sulphur, - -	0·0248 gram.
" pyrites and burnt bones. " -	0·0221 "
	0·0027=0·135 p. c.

On comparing the results of the analyses, there is found in the one case a loss of sulphur equal to 0·228 p. c. of the weight of the iron, and in the other 0·135 p. c. in remelting with pyrites in the presence of phosphorus, since the two fusions of each iron were made in equal quantity of pyrites, and the loss of sulphur was greatest where the calcined bones were added.* I think therefore that this loss of sulphur was owing to the combination of a portion of this last, with the phosphorus introduced after fusion, these two bodies, in consequence of their great affinity, combining in all proportions to form some very volatile compounds.

To establish undeniably whether this probable combination had taken place, I made some reverse synthetic experiments; that is to say, I remelted:

1st. 5 grammes of a gray iron with 0·20 of pyrites, 0·20 of calcined bones and 0·10 of clay.

2d. 5 grammes of the same iron with 0·10 clay and 0·20 calcined bones, to see what became of the phosphorus after the second fusion in the presence of pyrites.

The two experiments gave me two buttons perfectly melted but entirely different in their external characters. The first, without being very ductile, flattened slightly under the hammer before breaking; its fracture was not smooth and shining as that of the button resulting from the fusion with the burnt bones without pyrites.

This last, on the contrary, was very friable, did not flatten under the hammer, broke at the first blow; it presented a perfectly smooth surface, was silver-gray, quite brilliant, without any fissure. Apart from its want of ductility it had many of the exterior characters of nickel.

* The sulphur in the first cast iron was not determined before fusion. It was of but little importance to know this quantity which was in proportion to the others in the two experiments of each iron. It was sufficient to know that it lost more by the addition of phosphorus.

So great a difference in the exterior characters of the two buttons was sufficient to make me believe in a loss of phosphorus through the presence of sulphur.

As a final test, I remelted again:

5 grammes of a gray cast iron with 0·20 of pyrites, 0·20 of calcined bones, and 0·20 of clay.

5 grammes of the same iron with 0·20 of calcined bones and 0·20 of clay.

Two grammes of each button from the fusion were analyzed.

* * * *

Result of experiments with calcined bones gave phosphoric acid, 0·024
" " " " calcined bones and pyrites, phosphoric acid, 0·027

It is seen from the two results of the analyses, that the phosphorus did not disappear in the fusion of the iron in the presence of phosphate of lime and pyrites, whilst the analyses in the inverse experiments showed a loss of sulphur which evidently could not have proceeded from a volatile combination of sulphur and of phosphorus as was first supposed.

M. Gruner, Engineer in chief of the mines, my excellent professor to whom I am indebted for numerous useful councils in this memoir, advised me to take again the first two buttons and examine the carbon, to assure myself if it did not play an important part in the disappearance of the sulphur.

He had in fact remarked, in an assay by the dry way of ores highly phosphatic, that some isolated granules of the fused metal which had no magnetic properties, rose to the surface owing to their feeble density, and which were, doubtless, phosphuret of iron without carbon; for had they been other than this, they would have been simply a phosphatic cast iron which would have possessed the magnetic properties of the fused button.

Indeed, it appears from the experiments which I have made, that phosphorus, tending to combine with the iron of the casting to form a phosphuret, replaces a certain portion of the carbon, which in its turn meets with sulphur from the pyrites and forms a sulphid of carbon independent of that which forms from the presence of pyrites alone without the concurrence of phosphorus.

In this manner we may explain the loss of sulphur, which has been proved to occur without a loss of phosphorus.

These analyses of carbon presenting great difficulties, particularly in the workshop of a metallurgist, I adopted the method of M. Regnault for the combustion of the cast iron, as it seemed the only convenient method of separating these small quantities of carbon.

In open crucibles by a forge fire were remelted:

1st. 10 grammes of very fine iron wire (No. 8 of commerce) cut into three small pieces, with 0·20 grammes of pyrites.

2d. 10 grammes of the same wire with 0·20 gr. pyrites, and 0·20 gram. of calcined bones.

The amount of sulphur in the two fused buttons were estimated in order to ascertain whether there had been a loss of sulphur as in the fusion of the cast iron, made under the same circumstances.

For if in these two trials the analyses gave the same amount of sulphur, it would be demonstrated that carbon was the sole agent which caused a loss of sulphur in the case of the fusion of the cast iron, and that the phosphorus by replacement of the carbon had assisted in this loss of sulphur. In melting the iron wire in the presence of pyrites and in an open crucible, it might be supposed that a loss of sulphur would occur by oxydation from the air, but such was not to be feared in this case, as a great quantity of iron covered the pyrites completely, preventing all contact with the air.

The two assays succeeded perfectly: the two buttons were well fused. That made with the phosphate of lime was very hard, breaking readily, from which it is seen that phosphorus tends to make iron very "cold-short." 1·90 grammes of each button of fusion were treated with boiling nitric and a little hydrochloric acid so as to dissolve everything completely, except some little granules of sulphur, which were readily collected.

The following are the results of the two assays:

1. Trial with pyrites and calcined bones;

Sulphur not attacked by acids, - - -	0·0150
Sulphate of baryta, 0·03 : Sulphur, - - -	0·0041
	0·0191

2. Trial made with pyrites alone;

Sulphur not attacked by acids, - - -	0·0001	
Sulphate of baryta, 0·065 : Sulphur, - -	0·0089	
	·0190	0·0191
		0·0190
		·0001

From this it is seen that there is no material difference in the quantity of sulphur, since on calculating the sulphur in the sulphate of baryta to five decimals, there is only a difference of a tenth of a milligram. It may therefore be concluded that phosphorus removes no portion of the sulphur when iron is remelted in presence of pyrites and phosphorus.

On the contrary, when cast iron is melted with pyrites and phosphate of lime, there is always found a loss of sulphur.

The only difference in the results is in the carbon; and it follows, as before stated, that the loss of sulphur is to be attributed to its combination with carbon; a combination facilitated by the presence of phosphorus, which tending constantly to form a phosphuret of iron replaces a certain quantity of carbon.

We thence naturally infer that irons are less "hot-short" which are obtained with a mixture of phosphatic ores, because of the *influence of phosphorus in neutralizing partially the action of sulphur in the irons.*

These explanations give us also an explanation of a known fact in practical metallurgy, the explanation at present given being insufficient although partly true. There is no practical metallurgist who in treating very phosphatic ores has not obtained, contrary to his expectation, with a proper heat and slags, indicating a complete reduction,—some cast iron completely white.

It has always been supposed that phosphorus gave too great fusibility to the mass and accordingly prevented the formation of a gray iron. But to this explanation, which has some truth, there must be added the loss of carbon caused by the tendency of phosphorus to form a phosphuret of iron. For if the fusibility were the only cause, the slags should be slightly charged with iron, which is not the case. In all the synthetical trials and analytical results obtained, we find that phosphorus serves to remove a portion of the sulphur probably by favoring the separation of the carbon of the cast iron which then combines with the sulphur to form a sulphid of carbon.

Moreover, phosphatic irons, according to Karsten (Manuel de Metallurgie), preserve better their heat, pass more quickly to a white heat, a heat which spreads uniformly through the whole, whilst sulphurous irons, and as a consequence those "hot-short," cool rapidly and very unequally. The bar of forged iron in this condition presents no homogeneity, and breaks very easily. Hence in certain cases the influence of phosphorus, which is generally considered very injurious, may perhaps be advantageous. The metallurgist should use it with moderation, avoiding too great a quantity in the bed of fusion, as it is known that after cooling completely, phosphatic irons are brittle.

It is therefore necessary before using it to ascertain the required amount by a series of experiments on a large scale. These experiments should be made with much care, for, according to Karsten, it is known that the extreme limit of phosphorus in a good iron is 0·30 per cent; if this quantity is exceeded the irons lose their tenacity when cold and will not resist a shock or blow.

Very phosphatic and "cold-short" irons have ordinarily a grained fracture, with brilliant facets, without any fibres like many strong irons. The practical metallurgist ordinarily selects them for uses where the very soft slightly "hot-short" irons

would be disadvantageous, as in the manufacture of nails There is often also a demand for certain purposes for brittle iron.

It has indeed been remarked, but without explaining the fact, that certain coarse-grained "cold-short" irons forge very easily. We must not conclude from this (which has often been done), that all "cold-short" coarse-grained irons are always good when hot. In fact, a bad iron may be charged with silicium for instance, which will make it "hot-short" as well as "cold-short," and which will not work nearly as well as a fibrous iron well prepared and free from silicium. It is then necessary to ascertain first that the irons are cold-short from the presence of a small quantity of phosphorus, and not from the presence of another ingredient, or some cause inherent in the metallurgical treatment.

In all the experiments on a grand scale, made to favor the passage of the phosphorus into cast iron without compromising its quality by introducing silicium, I have always observed that the result from a charge containing equal quantities of clay and carbonate of lime was the best. In this case the slags have the following composition:

		Oxygen.	
Silica,	48·07 pr. ct.	24·97	24·97
Alumina,	15·94	7·44 }	17·55
Lime,	35·99	10·11 }	

These slags are well suited to a good result; for without being too silicious they are not injurious to the quality of the iron, in facilitating the reduction of the silica; neither do they contain too great a quantity of base to prevent the decomposition of the phosphates in the bed of fusion and the passage of the phosphorus into the cast iron.

In conclusion I will allude here to the memoir of M. Stengel, (Ann. des Mines, t. x, 3d série), who pretends to have assayed very sulphurous irons which were not in the least degree "hot-short." He contends that it is to copper that this property of iron must be attributed. Every one knows in fact that a feeble quantity of copper prevents iron from welding and renders it "hot-short" in a high degree. But I will say in opposition to M. Stengel, that this quality cannot ordinarily be attributed to copper, copper being very rarely present, but as a general thing, to the presence of sulphur. Besides, in all the trials on a large scale which I have made with care, if copper had been the cause of the "hot-short" iron which was obtained, there would have been "fers de couleur" at the time of treatment with charcoal as well as when the mineral coal was used, which was by no means the case.

When the irons obtained in la Bourgogne and la Franche-Comté by the use of charcoal and those derived from the same ores treated with mineral coal in the furnace of la Loire or the

Rhône, are compared, vast difference is found, the latter being very "hot-short," owing to the pyrites in the coal.

Recapitulation.—From the researches, experiments and analyses in this memoir, it will be seen:

1st. That traces of sulphur are sufficient to give a bad quality to iron and render it "hot-short."

2d. That the passage of sulphur into the iron takes place generally in the fusion of the ore in contact with mineral coal in a high furnace, and consequently it is in this operation that it must be counteracted.

3d. That phosphorus in the "bed of fusion" removes a portion of the sulphur in the cast irons by replacing a portion of carbon, and facilitating accordingly the formation of sulphid of carbon.

4th. That the quantity of phosphorus is not diminished when cast iron and irons are melted in presence of pyrites.

5th. That phosphorus in irons facilitates their being worked hot, rendering them harder and more difficult to cut.

6th. That it is necessary to study well the elements in the bed of fusion, so as not to introduce into the irons too great a quantity of phosphorus so as to render them too brittle when cold by its excess.

ART. XXV.—*On a Subsidence of the Land on the Sea-coast of New Jersey and Long Island;* by Professor GEO. H. COOK, of Rutgers College, New Jersey.

(Read before the American Association at Montreal, Aug. 13th, 1857.)

IN the course of some geological examinations along the coast of Southern New Jersey, my attention was frequently called to various facts indicating a change in the relative level of the land and water, at some recent period. An attentive examination of these facts has led me to the conclusion, that a gradual subsidence of the land is now in progress throughout the whole length of New Jersey and of Long Island; and from information derived from others I am induced to think that this subsidence may extend along a considerable portion of the Atlantic coast of the United States.

The occurrence of timber in the marshes and water below tide-level is common along our whole Atlantic shore. Almost every person at all familiar with shore life has observed the remains of logs, stumps and roots in such places. Generally, however, they have been looked upon as the remains of trees, torn from their original places of growth by torrents or by the wearing away of the shores, and deposited where they are found by the ordinary action of the water. To any one who examines

them carefully it soon becomes evident that they grew upon the spots where they now are. The stumps remain upright; their roots are still fast in the firm loamy ground which underlies the marsh—and their bark and small roots remain attached to them. The localities, too, where they are most abundant, are such as are least liable to be affected by the violent action of the water or of storms. Thus they are by far the most abundant on the low and gently sloping shores of Long Island, New Jersey, and all the states farther south, which are protected from the violent action of the surf by a line of sand beaches, at the same time that the numerous inlets allow free access to the tides. In these protected situations, hundreds and even thousands of acres can be found, in which the bottoms of the marshes and bays are as thickly set with the stumps of trees, as is the ground of any living forest.

The first and the chief part of my own observations were made upon the southern part of New Jersey, following the shore of Delaware Bay from its head down to Cape May, and the Atlantic shore from Cape May north to Great Egg Harbor. The examinations have since been continued along the shore to New York City, and thence eastward at several points along the south shore of Long Island.

In the marshes above Salem, great numbers of the stumps and trunks of trees are met with in digging ditches at all depths quite down to the solid ground.

It will be remembered in the frequent allusions made to the marsh, that its surface is nearly on a level with high tide mark, and, of course, that whatever is buried in it is below that mark.

At Elsinboro Point, a little farther down on the Delaware Bay shore, the cutting away of the marsh by the water has left great numbers of stumps exposed, and they can be seen at every low tide, still firmly rooted in the hard ground. In the bank of Alloways Creek, a few miles below, the remains of trees can be seen under the same circumstances. They are common in all the marshes of Cumberland Co. Great numbers of them can be seen in the marshes on Maurice River at Dorchester and below.

In Cape May county they are observed everywhere in the marshes and creeks; good localities are to be found at Dennisville on the Delaware Bay, on the inside of Seven-mile Beach, on the sea-side, and below Tuckahoe on Great Egg Harbor.

They are common in the marsh which extends along the whole shore back of the beaches quite to Squan. Great numbers of them can also be seen at low water on the strand at Point Comfort, opposite Sandy Hook.

In the marsh on the Raritan above South Amboy, hundreds of them were dug out in cutting a canal across a bend in South River.

The marshes about the mouths of the Passaic and Hackensack rivers are filled with the remains of cedar timber.

The marshes on Staten Island also contain buried timber; and on Long Island at Hempstead, at Babylon, at Islip, and still farther east, the same fact is of constant occurrence.

I may remark that the remains of trees are not equally abundant in all localities, owing partly perhaps to differences of exposure, but more to the difference in durability of the various species of wood. In many places where oak, gum and other deciduous trees were known to stand formerly, there are no traces of them now; they have entirely rotted away. On the contrary, the pine and the red and white cedar are almost indestructible. I have seen pine stumps several feet under the marsh, where they have been for an unknown period, which retain the characteristic smell and appearance of the wood almost as perfectly as the fresh cut specimens. At several places in southern New Jersey an enormous amount of white cedar timber is found buried in the salt marshes, sound and fit for use, and a considerable business is carried on in mining this timber and splitting it into shingles for market. In some places it is found so near the surface that fragments of the roots and branches are seen projecting above the marsh, while in other cases, the whole is covered with smooth meadow sods, and there is no indication of what is beneath till it is sounded by thrusting a rod down into the mud.

It is in deposits where these durable species of wood are found that we get the most accurate idea of the depth to which these remains extend. At Dennisville, there is a large tract of marsh underlaid by cedar swamp earth and timber. By probing the marsh with an iron rod the workmen find where the solid timber lies, and then, removing the surface sods and roots, they manage to work in the mud and water with long one-handled saws, and cut off the logs, which, as soon as they are loosened, rise and float, and of course are easily managed. The timber is not water-logged at all, but retains its buoyancy, and the removal of that nearest the surface releases that which is below and it rises, so that a new supply is constantly coming up to the workman. In this way a single piece of swamp which is below tide level has been worked for fifty years past and still gives profitable returns. The timber is found lying in every direction, some appearing to have been blown down by the wind, and some to have died and fallen after it was partially decayed. The fallen timber has been covered by the accumulation of muck from the decayed leaves and twigs, and other timber has grown on this, to fall and in its turn give place to still another growth. How long this accumulation has been going on it is impossible to tell. Dr. Beesley of Dennisville counted 1080 rings of annual growth

in a stump, and lying directly under this, so that it must have fallen before this grew, was a log with 500 rings. I have seen them lying in this way, log under log, indicating that thousands of years must have passed while they were accumulating. And this is only the superficial portion of it. In some of the creeks in the marsh, there are hundreds of stumps which only fall bare at extreme low water, and both in Dennis Creek and in Maurice River there are fast stumps on which there is seven or eight feet water at low tide. With an instrument for sounding marshes, I have been able to find these logs as low as eighteen feet below the surface of the marsh; and the material brought up from that depth was cedar swamp earth. In digging a well in the marsh at Dennisville, a foot or two of marsh mud was passed through, and then swamp earth or muck quite down to the gravelly bottom, which was reached at eleven feet. Most of the timber found was cedar, but at the bottom and fast in the hard ground was gum and magnolia.

Facts of the kind mentioned above are by no means confined to the shores of New Jersey and Long Island. Prof. Hitchcock, in his Geology of Massachusetts, p. 307, says a "submarine forest exists at Holmes's Hole, on Martha's Vineyard. It is on the west side of the harbor, and was described by the pilot as having the appearance of a marsh at low water. Stumps have been found there in considerable quantity, of the cedar at least.

"Near the southwest extremity of the Vineyard, on the north shore, I was informed that another forest of similar description may be seen. On the north side of Cape Cod, also, opposite Yarmouth, cedar stumps may be found (as I was informed by the captain of the Falmouth packet) extending more than three miles into Barnstable Bay. And Mr. Henry Wilder, of Lancaster, who first directed my attention to this subject, says that the same thing occurs in the Bay of Provincetown, on the side opposite the village."

Sir Chas. Lyell, in his Second Visit to the United States, vol. i, p. 33, in speaking of the country about Portsmouth, N. H., says, "it is a curious fact that we discover along this eastern coast unequivocal signs of partial subsidence of land at a recent period. The evidence consists of swamps, now submerged at low water, containing the roots and upright stools of the white cedar, showing that an ancient forest must once have extended farther seaward. One of these swamps we passed yesterday at Hampton, on our way from Boston to Portsmouth."

In Dr. Jackson's Report on the Geology of New Hampshire, p. 280, an article by J. L. Hayes of Portsmouth, mentions that at Rye Beach, "among the curious phenomena which may be witnessed, are the remains of a submerged forest. The stumps and roots of trees of a large size, are frequently seen during very low

tides, on the lower margin of the beach. They appear to have been broken off near the roots, which remain in their original positions. The trees were mostly white cedar. . . . . Stumps of trees are found in most of the salt marshes around Portsmouth."

In the second volume of Lyell's First Visit to the United States, p. 143, where he speaks of the flats on the Bay of Fundy, he says, "I was informed that there was a submerged forest buried in the red mud, and exposed to view in the Cumberland Basin at low tide, not far from Fort Cumberland."

Prof. Dawson, in a paper, On a Modern Submerged Forest at Fort Lawrence, Nova Scotia, published in the Quart. Journ. Geol. Soc., vol. xi, p. 119, mentions that, "outside of the edge of the marsh, and about twenty-five feet below the level of the highest tides, which here rise in all about forty feet, ... appear erect stumps and many prostrate trunks of trees. The stumps are scattered as in an open forest, and occupy a belt of 135 paces in breadth, and extending on either side for a much greater distance. I saw more than thirty stumps in the limited portion of the belt which I examined. . . . On digging around some of the stumps, they were found to be rooted in ground having all the characters of ordinary upland forest soil. . . . . The smallest roots of all the stumps were quite entire and covered with their bark, and the appearances were perfectly conclusive as to their being in the place of their growth. . . . . All the stumps and trunks observed were pine and beech, and it is worthy of notice that these are trees indicative rather of dry upland than of swampy ground."

Edmund Blunt, Esq., of New York City, informs me that he had observed the various phenomena connected with the submerged timber, on the south shore of Long Island, and on the shores of Delaware and Chesapeake Bays, and that he made them the subject of a verbal communication to the New York Lyceum twenty years ago.

Dr. Emmons, State Geologist of North Carolina, has observed submerged forests under similar circumstances in Albemarle Sound.

Lyell, in the account of his First Visit to the United States, vol. i, p. 131, says, "at Beauly, in Georgia, I found upright stumps of trees of pine, cedar, and ilex, covered with live oysters and barnacles, and exposed at low tide; the deposit in which they were buried having been recently washed away from around them by the waves." And at p. 139, "near the mouth of Cooper River in S. Carolina, in the marshes, there are deep deposits of clay and sand enclosing the stools and trunks of the cypress, hickory, and cedar, often imbedded in an erect position, which must have grown in fresh water, but are now sunk six and even sixteen feet below the level of high water."

Wm. Bartram, in his Travels in North and South Carolina in the last century, says, "It seems evident even to demonstration that those salt marshes adjoining the coast of the main, and the reedy and grassy islands and marshes in the rivers, which are now overflowed at every tide, were formerly high swamps of firm land, affording forests of cypress, tupelo, Magnolia grandiflora, oak, ash, and sweet bay, and other timber trees, the same as are now growing on the river swamps, whose surface is two feet or more above the spring tides that flow at this day. And it is plainly to be seen by every planter along the coast of Carolina, Georgia and Florida to the Mississippi, when they bank in these grassy tide marshes for cultivation, that they cannot sink their drains above three or four feet below the surface, before they come to a stratum of cypress stumps and other trees, as close together as they now grow in the swamps."

There is another class of facts somewhat similar to those above-mentioned, and of common occurrence along our shores, from which these should be distinguished. The facts to which I refer are such as the following. At Cape Island, Cape May county, there are found stumps of oak trees at tide level which have been covered by twelve or fourteen feet of upland soil—cultivated farm land—and have but recently been exposed by the wearing away of the shores. At Union, on Raritan Bay, in solid earth and about two feet below low water, common hard wood stumps were found, in digging a large basin. Upright stumps of trees have also been found in digging wells on the upland, at numerous places near tide water, on Delaware Bay and the Atlantic shores. In similar localities, shells of the common clam, oyster, and other recent species have been found in wells, and I have observed them at various places several feet above high tide.

In the bank of Maurice River, seven or eight feet above high water and still covered by several feet of sandy earth, is an oyster bed. It is exposed for some rods. The shells are in common blue mud, closely wedged in together, and standing with the opening of the valves upwards, just as in the living beds. At Tuckahoe, casts and impressions of the common clam are found in the gravel at eight or ten feet above high water. And at Port Elizabeth and near Leesburgh, shells of the clam and oyster, and indeed of nearly all the species of shells now common in the bay are found, covered by from two to six feet of sandy loam, and are extensively dug for manure. I was lately informed of the existence of an oyster bed under similar circumstances on the beach a little north of Long Branch. Deposits of recent shells are found in much the same way, on all our Atlantic coast, and also on the Gulf of Mexico. So many have been given by different observers, that for the present purpose it is

not necessary to specify them. Attention is called to them now as indications of a period of subsidence, and then one of elevation preceding the present.

The fossils, it will be perceived, are in circumstances which require that the ground should have occupied a much lower relative level than the present, and the covering which is over them is upland soil,—portions of that in New Jersey are in cultivation,—and are among the most valuable and productive soils in the state. While on the contrary the remains of trees, &c. which are specially referred to in this paper are all as low as the present level of high tide and are covered only by water or by marsh mud and roots. They are also of a much more recent date, some of them having been growing trees within the memory of persons now living, and the subsidence which has produced them is one that is still in progress as I wish now to show.

In referring to the facts it will be convenient to follow the same geographical order which has been observed before.

In Elsinboro, on Delaware Bay, a portion of what is now tide meadow was a heavily timbered maple swamp since the country was first settled, and after this had died out and been replaced by marsh there still remained in the midst of it a knoll or island, as it is locally termed, of hard ground covered with timber. Now this timber too is all dead, and the island is lost in the marsh. The inhabitants say the tides run higher on the upland than they formerly did. It manifestly is not caused by any washing away of the soil. On Alloway's Creek there was a considerable island in the unbanked marsh which was cultivated for corn, wheat, and other farm crops within the present century; but now it is overflowed by high tides, and is covered with marsh mud and grass. A surveyor in extensive practice,* in Cumberland Co. west of the Cohansey, informs me that there are a number of such instances which have come under his own observation. He referred me to one island of considerable size, which in the early survey of the country is described and mapped as being covered with timber. No timber is now to be seen there, and the island is no longer upland. Its place is only ascertained by sounding the marsh, when it is recognized by the shallower deposit of mud and roots on it. Other instances of the same kind were mentioned to me on the east of the Cohansey, of precisely the same character.

The owner of an extensive tract of land between Maurice River and West Creek† informed me that within the last 50 years he had lost 1000 acres of timber,—by the tides running higher on the upland than they formerly did. On Stipsons Island I was shown portions of upland on which good crops of

* Belford M. Bonham, Esq. † Joshua Brick, Esq.

wheat had been raised within 30 years; now it is liable to be overrun by the tide, and cannot be cultivated. In the last 50 years this farm which is of perhaps 200 acres, has lost 50 acres, part wood and part cultivated land, from this cause.

At Dennisville, and indeed along the whole of the Bay shore of Cape May, the same fact is to be observed. Wherever the shores are low and the slopes gentle, the lower edges of the woodlands and also scattering trees near the marsh are liable to be killed by the tide, and every year shows advances of the water upon the land by the dead trees which it leaves,—and which are never replaced by a new growth of young timber, but by salt marsh.

On the sea-side of Cape May the shore is bolder and of course the encroachments are less perceptible, but even here numerous instances were related to me, by persons who had observed them, of the killing of timber by the tide,—and on the west side of the beaches I saw great numbers of dead red cedar trees still standing in the marsh; the earth in which they grew covered by a few inches of mud,—and liable to be overflowed by high tides. The dying out of oak and other hard-wood timber was also shown to me at Barnegat. On Squan Beach there is a considerable tract exposed at about half tide, from which the timber was cut off sixty or seventy years ago, and the stumps are standing yet. At South Amboy on Raritan Bay I saw three or four dead white oak trees, all that were left of a grove of 150 trees which flourished there thirty or forty years ago. They were on a slope which was formerly covered with grass, but the sandy gravel has been driven in by the storms,—the grass and trees are killed. Capt. Peterson an old inhabitant of Washington on the Raritan, says that a portion of what is now salt marsh was formerly a swamp; and that a sandy point projecting into and partly covered by marsh was within his recollection covered with pine trees.

At Hempstead on Long Island, an old resident, Mr. Valentine Smith, informed me that he knew of an island, in his meadow, which was covered with trees since his recollection, but which is now salt marsh; and the highest part of the island is lower than the surface of the meadow.

The district of country where the principal part of my observations have been made is particularly favorable to the detection of changes of level in the tide waters, on account of its low shores, and its being so generally protected from the direct action of the waves or of currents. Most of the Jersey shore of Delaware Bay and of the Atlantic is fringed by a strip of salt marsh, in many places several miles wide. High tides flow over these it is true; but not to any considerable depth, so as to produce currents that could wear the upland, and they do not appear

to wear it at all. The land is very low and level. In the whole county of Cape May there is not a hill of any magnitude. To give an idea of the uniformity of its surface I may mention that a railroad line twenty-four miles long, was surveyed through the central and highest part of the county, in which the greatest elevation passed over was twenty-eight feet above high water, and the average was but eleven feet. The land near the shores in the adjoining counties is equally low. On such shores it will readily be perceived that a very slight depression of the surface must bring a broad strip of land under water and that marks of such depression will be found in much greater abundance than in localities where the shores are bolder.

The people along the shore in such places are very sensible of this change of level between the land and water, and are perfectly well satisfied that the remains of the timber found are in the places where they grew, and that they have not gone down by the ground washing away, or becoming more compact. When it was objected to them that the white cedar trees have no tap roots but grow directly upon the muck, and of course that they might have settled; it was readily admitted that one might think so, but for the fact that when the cedar grows so that its roots can reach hard ground as they can when the swamp is shallow, that then the timber is worthless on account of the fibres interlocking so that it cannot be split into shingles, and that in shallow swamps and in the bottoms of the deeper swamps, such timber is found, which is to them a plain evidence that it grew there. Further they find at the bottom of such swamps gum and magnolia trees which have grown upon the hard ground. Pine stumps are also found at considerable depths below the surface; these are tap-rooted, and their roots reach the solid ground so that they are not liable to settle. It is the general impression however, that the cedar swamps do not settle as long as they remain constantly wet.

Many of the residents who have observed this gradual encroachment of the water and marsh on the upland, have accounted to themselves for it by a variation in the rise of the tides;—saying that the mouth of Delaware Bay and of the Inlets on the sea-shore are more obstructed than formerly and thus cause the tides to rise higher. I do not know whether the obstructions are as they state, but if they were so as to affect the flow of the tide, high-water mark would be lowered and not raised. But this is probably not of much importance, as the tide at Cape May rises only about five feet, and the changes of level which have been mentioned are more than equal to the whole of this.

Mr. Edmund Blunt of New York who agrees with me in regard to the apparent subsidence, is disposed to account for it by supposing that it is in some way dependent on the clearing and

cultivation of the ground, and that it ceases about buildings and other structures. It will be perceived, however, that many of the cases I have cited are of wood land which has been overrun by water before it was cleared.

Prof. Tuomey in his Report on the Geology of South Carolina refers to numerous facts to show that the opinion of a subsidence of the coast of that state, originally advanced by Bartram in his Travels, and since sustained by Lyell in Proc. Geol. Soc., vol. ii, p. 406, is incorrect. He concludes that "there is not a single instance of submerged swamp that cannot be traced to the encroachment of the ocean, without supposing any change in the relative level of land and water on the coast.

"Those writers who have referred them to the latter cause erred in not having first studied the nature and level of the swamps in which the trees grew whose stumps are found submerged, and in not distinguishing tap-roots from true stumps."

I do not think the cases to which I have referred are open to this objection, as many of them were of upland forests and not swamps at all.

Prof. Hitchcock in a paper on the Geology of Portland and vicinity, (Bost. Jour. Nat Hist., vol. i, p. 338,) and J. L. Hayes, Esq., before quoted, do not think the instances they have cited are proofs of subsidence, but that they are only low lying swamps, originally shut in from the salt water, but which by the wearing away of the shores have been exposed to the action of the sea which has washed out the muck and lowered the stumps. Such an explanation would not apply to the cases of the islands which I have mentioned or to those along the sloping shore.

. Mr. J. W. Dawson, in an article which I have before quoted, after mentioning a popular explanation connected with some observations at the mouth of the Bay, but which he thinks insufficient, says: "The only remaining mode of accounting for the phenomena, is the supposition that a subsidence to the amount of forty feet has occurred in the district. Such a subsidence is not likely to have been limited to Fort Lawrence Point, and accordingly I have been informed by intelligent persons long resident in the neighborhood that submerged stumps have been observed at a number of other places in circumstances which showed that they were *in situ*, and that trees and vegetable soil have been uncovered in digging ditches in the marsh. Nor are these appearances limited to Cumberland Basin. At the mouth of Folly River, on the southern arm of the Bay, a submerged forest on an extensive scale is said to occur, and in the marshes at Cornwallis and Granville vegetable soils are found under the marsh. These facts render it probable that the subsidence in question has extended over the whole shores of the Bay, and that the marshes have been deposited, and the present lines of coast-cliffs cut, since its occurrence.

"The marshes of the Bay of Fundy are known to have existed at or about their present level for 250 years. It is true that an opinion prevails in some of the marsh-districts that the tides now rise higher than formerly, and in proof it is alleged that the dykes are now maintained with greater difficulty, and that tracts of marsh once dyked have been abandoned. The settling of the mud and the narrowing of the tidal channels by new embankments may however have produced these effects. For the antiquity of these submerged forests we must therefore add to the two centuries and a half which have elapsed since the European occupation of the country, a sufficient time for the deposition of the alluvium of the marshes. On the other hand the state of preservation of the wood, after making every allowance for the preservative effects of the salt mud, shows that its growth and submergence must belong to the later part of the modern period."

From the pilots who go out of New York harbor I have not been able to learn that they have observed much variation in the depth of water in the channel, but they say that they probably should not notice it. The report of the Harbor Commissioners shows that local causes are in operation which affect the harbor of New York much more than this regular and slow one. Two intelligent pilots who go out of the Raritan River inform me that there is a greater depth of water in that river than there was thirty years ago.

After examining all I have been able to find written upon the subject, and after studying it in the field, I can think of no other theory which will apply to all the facts except that of a slow and continued subsidence of the land.

The rapid wear of the shores may fairly be adduced as confirming my conclusions in regard to subsidence. A few cases of this rapid wear may be given. Egg Island, a point well known to those who are familiar with Delaware Bay, is put down on the first map made by the proprietors of West Jersey in 1694 as containing three-hundred acres of land. It now contains only about three-fourths of an acre at low water, and high tides cover it entirely. I am assured by a surveyor* who has long been engaged in his business, and has traced out many old lines which formerly terminated on the shore, that the wear of the salt marsh along Maurice River Cove since the first settlement of the country, is not less than three-fourths of a mile. At the mouth of Dennis Creek the wear is very great—forty rods have been worn off in three years,—and boatmen there tell me that the distance from Dennisville to the Bay is less by a mile than it was when they first knew the creek. At Town Bank the upland comes out to the shore in a bold bank from fifteen to twenty feet high,

* James L. Smith, Esq.

The first settlement in Cape May county was made at this spot, but the site of the buildings, and a grave yard which was back of them have been entirely worn away. The light house at the Cape has been moved inland considerably, on account of the wearing away of the shore. At Cape Island about the year 1780, what is now the bathing ground and on the ocean shore, was a mile inland, and the space between the houses and the shore was cultivated ground. Poverty Beach, now a short sand bank, was then Four mile Beach and extended from Cold Spring Inlet to the point of Cape May.

The beaches farther up the shore which are exposed to the direct action of the sea wear away rapidly, and where they are narrow and not covered with timber they drift in towards the land. In some cases they have moved in their entire breadth and now rest on the salt marsh which formerly was behind or to the west of them. This wear is common along the whole of the Jersey shore. Capt. J. W. Herbert a very intelligent wreck master at Keyport has a number of marks on the beaches set to determine the location of sunken vessels, and from these he is able to measure the wear from year to year, and the average which he deduces from these is not less than twelve feet a year along the whole shore. He verifies these measurements further by the position of the vessels themselves; some of them were buried in sand in the bar which is outside of the shore and parallel with it, and others were inside of this. After a few years these vessels are found outside of the bar, in deep water and clear of sand; the shore has worn away and the bar has followed it in.

At Long Branch, which is hard upland, the wear is very serious. The spot where the first boarding house was located, thirty years since, together with the road which ran behind it, is now all worn away, and the shore line is to the west of it. The wear is irregular; last year it was from twelve to twenty feet. Along the shore of Sandy Hook and Raritan Bays the wear is equally rapid. At the Highlands enormous slides have been the result of this wear. At Union the washing away has averaged three or four feet a year for three years past. At the mouth of Cheesequakes the channel is now where there were cultivated fields sixty or seventy years ago. The elm tree, formerly a prominent land-mark on the south shore of Staten Island, is gone and it is said the water is of some depth where it stood. On Long Island the wear of the beaches is not so uniform, but is perceptible. On the east end of the Island the wear is very great and has attracted attention ever since the first settlement of the country, for details of which reference may be made to Mather's Geol. Rep. in Nat. Hist. of New York.

In regard to *the rate at which this subsidence is going on*, I may state a few facts. On Oldman's Creek is a tide meadow

which was banked in soon after the settlement of the country, perhaps 150 years ago. The sluices in this bank are now full three feet below low-water mark, so low that they are entirely useless, and it is a puzzle to the inhabitants to know what they were ever put there for. The meadows on the north side of Alloway's Creek were also banked about 150 years since. The sluices in these banks are full three feet below low tide, and useless. I am assured by those who are familiar with such sluices that they are not liable to settle in the mud when properly placed. On the south side of the same stream, where the hard upland comes to the waters edge, the stump of a tree is standing, which has been cut down since the settlement of the country, and now, though it is still firm in the gravelly and tenacious soil of the hard ground, every high tide runs over it. The very intelligent farmer* who pointed it out to me, said there were several others of the same kind farther down the stream. The wood was oak; standing as they did, he thought they would have been killed by the tide before it came within three feet of its present height. On the Cohansey at Bridgeton, Mr. David Reeve who was concerned in the iron works there, made observations upon the tides for twenty years from 1816. The log on which he marked the result of his observations is now lost; but he says confidently, "that the tides uniformly, when uninfluenced by winds, rose higher than when my observations commenced, but how much higher I am now unable to inform you."

At Bateman's mill on Cedar Creek in the same county, the miller assures me that the tides rise eight inches higher on his wheel than they did twenty-five years ago.

The Hon. Joshua Brick of Port Elisabeth, who is noted as an accurate observer, gives it as his judgment that the tides rise upon the upland a foot higher than they did fifty years ago.

A water mill at the head of tide on West Creek, as I am informed by Benjamin Goffe, Esq., was very carefully located so that the wheel should not be affected by the tide backing up against it. It was built fifty-two years ago, and then it was very rare indeed for it to be stopped by the tide. Now it is stopped a number of times every year, and it is the opinion of those most competent to judge, that the tide rises fifteen inches higher on the wheel than it did formerly, and they are sure it is not less than twelve inches.

A water mill standing on Sluice Creek has been built 100 years, and the owner† informs me that from old papers in his possession he thinks it has lost two feet of head,—indeed it is only kept running by a dam and sluice some distance below.

* Mr. Thomas Shourds. † Clinton Ludlam Esq.

A tide mill on the sea-side, about two miles below Beeseley's Point, was attended by the same miller* for twenty-five years, and he informs me that he knows he lost four inches in the head of water in that time, and may have lost more.

From these several results—three of a subsidence of *three feet* in one hundred and fifty years, one of *two* feet in one hundred years, two of *one* foot in fifty years, and one of *four inches* and one of *eight inches* in twenty-five years,—we may with some degree of probability, set the average subsidence in the district where the observations were made, at two feet in a century.

With the exception of the statements of two pilots upon the Raritan River I have nothing upon which to base any estimates for the present rate of subsidence in the vicinity of New York. One of the pilots founds his conclusion upon observations made upon the wharf at Washington, and he is confident there is eight inches more of water there than there was twenty-five years ago. The other draws his conclusions from the depth of water upon the reef of rocks in the river below New Brunswick, and the depth upon the middle ground near Amboy, and from the action of the centre board of the vessel which always touches at these points, he is satisfied that the water is deeper than it was thirty years since; but he thinks not six inches deeper. The opportunities for accurate observation are much less frequent here than in the southern part of New Jersey, but from the phenomena of the marshes and of the submerged forests on Long Island and in northern New Jersey, I should infer that there was no material difference in the rate from that already deduced.

This paper is presented for the purpose of calling attention to the remarkable and important phenomena of which it treats. I purpose to continue the investigation, and shall be glad to receive statements of facts bearing upon the subject; either for or against the conclusions to which I have arrived. Facts in relation to the *rate* of subsidence are particularly desirable. I have given all that have come to my knowledge, and they agree singularly well with each other; but it is not unlikely that other and more numerous observations may give a different result.

New Brunswick, N. J., August, 1857.

* Mr. Nicholas Godfrey.

ART. XXVI.—*Biographical Memoir of William C. Redfield;** by Professor DENISON OLMSTED, of Yale College.

[From a Discourse delivered before the American Association for the Advancement of Science, at Montreal, August 17th, 1857.]

MR. WILLIAM C. REDFIELD, the well known author of a theory of Storms, and of various other valuable contributions to science, died at his residence in New York, February 12, 1857. A life passed in the ordinary walks of business, or in the quiet of philosophical research, affords little of that romantic incident which lends a charm to biography; still we think the life of Mr. Redfield will be found interesting and instructive in a three-fold point of view,—as affording a marked example of the successful pursuit of knowledge under difficulties,—as happily illustrating the union, in the same individual, of the man of science with the man of business,—and as exhibiting a philosopher, whose researches have extended the boundaries of knowledge, and greatly augmented the sum of human happiness.

William C. Redfield was born at Middletown, Connecticut, on the 25th of March, 1789. He was of pure English descent both by the father's and mother's side. His father from a natural love of adventure, chose in early youth a sea-faring life, and afterwards followed the seas as a profession to the time of his death, which happened when this, his eldest son, was only thirteen years old. His early training, therefore, devolved chiefly on his mother, who was a woman of superior mental endowments, and of exalted Christian character.

The slender pecuniary resources of the family would not allow young Redfield any opportunities of school education beyond those of the common schools of Connecticut, which, at that time, taught little more than the simplest rudiments—reading, spelling, writing, and a little arithmetic; and all access to the richer treasures of knowledge seemed to be forever denied him, when, at the early age of fourteen, he was removed to Upper Middletown, now called Cromwell, and apprenticed to a mechanic, whose tasks engrossed every moment of his time except a part of his evenings. These brief opportunities, however, he most diligently spent in the acquisition of knowledge, eagerly devouring every scientific work within his reach. He was denied even a lamp for reading by night, much of the time during his apprenticeship, and could command no better light than that of a common wood fire in the chimney corner. Under all these disadvantages, it is evident that before he was twenty-one years of age he had acquired no ordinary amount and variety of useful knowledge.

* We give place to this sketch of the life and scientific labors of the late Mr. Redfield with the more pleasure, since the earliest and the largest portion of his subsequent philosophical papers have been published in this Journal.—EDS.

During the latter part of his apprenticeship he united with other young men of the village in forming a debating society under the name of the "Friendly Association," with which was connected a small but growing library. To this humble literary club, Mr. Redfield always ascribed no small agency in inspiring him with a love of knowledge, and a high appreciation of its advantages; and during his future years, he nursed and liberally aided by his contributions this benefactor of his youth.

Fortunately for young Redfield, a distinguished and learned physician, Dr. William Tully, fixed his residence in the same village, and generously opened to him his extensive and well-selected library; and what must have been equally inspiring to youthful genius, Dr. Tully furnished him with a model of an enthusiastic devotee to knowledge, and of a mind richly stored with intellectual wealth. The modest youth who first presented himself as a suppliant for the loan of a book from the Doctor's library, was soon recognized as a congenial spirit, and was admitted to an intimate friendship, which lasted to the day of his death. Dr. Tully has favored us with the particulars of his first acquaintance with our friend. On his application for a book to occupy such moments as he could redeem from his daily tasks, the Doctor, being then ignorant of his acquirements or his taste, opened different cases of his library, submitting the contents of each to his selection. Among a great variety of authors, that which determined his choice was Sir Humphry Davy's Elements of Chemistry. As this was one of the earliest systematic works that contained the doctrine of Chemical Equivalents, a subject then considered as peculiarly difficult, and one understood by few readers of the work, the Doctor had little expectation that his young inquirer after knowledge, would either understand or relish it. In a short time he returned the book, and surprised the Doctor by evincing a thorough acquaintance with its contents, and expressing a high satisfaction, in particular, with the doctrine of chemical equivalents, which, he said, he had then met with for the first time.

Some time before young Redfield reached the end of his apprenticeship, his widowed mother had married and removed to the state of Ohio. He was no sooner master of his time than he set out on foot to pay her a visit in her new home, distant more than seven hundred miles. It was a formidable undertaking, in that early period before the age of steamboats and railways, and when a large part of the way was covered with dense forests, with hardly an open path even for the pedestrian. Stage coaches, indeed, ran on the nearer portions of the route, but these were too expensive for the slender finances of our young adventurer. Accompanied, therefore, by two other young men, he shouldered his knapsack and commenced the arduous journey. Every evening he noted down the incidents and observations of the day.

This journal is now in my possession, and I have perused it with deep interest for the graphic sketches it contains of the countries he passed through, then mostly new settlements, and for the indications it affords of those powers of observation, which afterwards led to the development of the laws of storms. The style of composition is far superior to what might reasonably have been expected from one who had enjoyed so few literary advantages, evincing two qualities for which Mr. Redfield was always distinguished—good sense and good taste. The sketches of Western New York, and of Northern Ohio, taken while the sites of Rochester and Cleveland were dark and gloomy forests, and Buffalo was a mere hamlet, possess no ordinary degree of historical interest. Instead of a "Lake Shore" road, traversed by the iron horse, as at present, our young pedestrians could find no better paths in which to travel over the southern side of Lake Erie, than to course along the beach. Yet in twenty-seven days they made good their journey, having rested four days on the way, making an average of about thirty-two miles per day. After passing the winter with his friends in Ohio, he resumed his way homeward on foot and alone, returning by a more southern route, through parts of the states of Virginia, Maryland, and Pennsylvania. We shall soon see to what valuable account he afterwards turned the observations made on these early pedestrian tours, in tracing the course as well as originating the project, of a great railway connecting the Hudson and the Mississippi rivers.

Returning to his former home in 1811, Mr. Redfield commenced the regular business of life. No circumstances could seem more unpropitious to his eminence as a philosopher, than those in which he was placed for nearly twenty years after his first settlement in business. A small mechanic in a country village, eking out a scanty income by uniting with the products of his trade the sale of a small assortment of merchandize, Mr. Redfield met with obstacles which in ordinary minds would have quenched the desire of intellectual progress. Yet every year added largely to his scientific acquisitions, and developed more fully his intellectual and moral energies. Meanwhile his active mind left its impress on the quiet community where he lived, in devising and carrying out various plans for advancing their social comfort and respectability, in the improvement and embellishment of their streets, school houses and churches, and in promoting the interests of the literary club, from which he himself, in early youth, had derived such signal advantages. From deep domestic trials which afflicted him about the year 1820, he had recourse for solace both to the word and the works of God. It was soon after one of the severest of these trials, that his attention was first directed to the subject of Atlantic Gales.

On the 3d of September, 1821, there occurred, in the eastern part of Connecticut, one of the most violent storms ever known there, and long remembered as the "great September Gale." Shortly after this, Mr. Redfield being on a journey to the western part of Massachusetts, happened to travel over a region covered by marks of the ravages of the recent storm. He was accompanied by his eldest son, then a young lad, who well remembers these early observations of his father, and the inferences he drew from them. At Middletown, the place of Mr. Redfield's residence, the gale commenced from the southeast, prostrating the trees towards the northwest; but on reaching the northwestern part of Connecticut, and the neighboring parts of Massachusetts, he was surprised to find that there the trees lay with their heads in the opposite direction, or towards the southeast. He was still more surprised to find, that at the very time when the wind was blowing with such violence from the *southeast* at Middletown, a *northwest* wind was blowing with equal violence at a point less than seventy miles distant from that place. On tracing further the course and direction of prostrated objects, and comparing the times when the storm reached different places, the idea flashed upon his mind that the storm was a *progressive whirlwind.* A conviction thus forced upon his mind after a full survey of the facts was not likely to lose its grasp. Amid all his cares, it clung to him, and was cherished with the enthusiasm usual to the student of nature, who is conscious of having become the honored medium of a new revelation of her mysteries. Nothing, however, could have been further from his mind, than the thought that the full development of that idea, would one day place him among the distinguished philosophers of his time. So little, indeed, did he dream of fame, that for eight or nine years after the first conception of his theory, he gave little attention to the study of the phenomena of storms, but was deeply engrossed in other enterprises which, although foreign to this subject, were alike evincive of his original and inventive turn of mind. Of these we may take a passing notice.

Before the scientific world, Mr. Redfield has appeared so exclusively in the character of a philosopher, especially of a meteorologist, that they have been hardly aware of the important services he has rendered the public in the character of *naval engineer*, particularly in the department of steamboat navigation. His attention was turned professionally towards this subject as early as the year 1820, when he became much interested in an experiment with a small boat propelled by an engine of new and peculiar construction, the invention of Franklin Kelsey, Esq., a townsman of his. Although the enterprize was not successful to the company, yet to himself it was not destitute of valuable results, as it was the occasion of his acquiring a more intimate

knowledge of the properties of steam, of steam navigation, and of ship building. On the ruins of that enterprize was erected another, which after some vicissitudes acquired a permanent success, and opened to him a sphere of professional labor which constituted ever afterwards the leading object of his life, as a man of business. Several disastrous steamboat explosions had spread alarm through the community and created a general terror of steamboats. Redfield was the first to devise and carry into execution the plan of a line of *safety barges* to ply on the Hudson between New York and Albany. The scheme was, to construct a passenger boat to be towed by a steamboat at such a distance from it as to avoid all apprehension of danger to the passengers. Large and commodious barges were built, fitted up with greater taste and luxury than had at that time been exhibited by steamboats. With these were connected two large and substantial steamers; and in the excited state of the public mind, these safety barges became great favorites with travellers, especially with parties of pleasure. But our countrymen never hold their fears long: a short interval of exemption from steamboat accidents ended the excitement, while the greater speed attained by the ordinary boats, and the lower fare, gradually drew off passengers from the safety barges, until they could be no longer run with profit to the company, and were abandoned. But the idea was not without profit, for it suggested to him the system of *tow boats* for conveying freight, which was established in the spring of 1826, and still continues under its original organization. The fleets of barges and canal boats, sometimes numbering forty or fifty, which make so conspicuous a figure on the Hudson river, were thus set in movement by Mr. Redfield, and for thirty years the superintendence of the line first established, constituted the appropriate business of our friend. In its management he employed unwearied industry, superior mechanical genius for contriving expedients, and a knowledge of both the science and art of steam navigation, such as was possessed by few men of business. Seldom have we seen the inductive philosopher so happily united with the practical engineer, each character borrowing aid from the other. Seldom has any one connected with the management of a steam navigation concern as his profession in life, carried into his business so much of the spirit of true science. No one else could have so thoroughly collected the statistics of the profession in this country, embracing all the facts relating to the explosion of steamboat boilers, as they successively occurred—the number of lives lost—the number of deaths by steam compared with those by lightning—and the number compared with those lost by other modes of travel. Moreover, while Mr. Redfield was diligently pursuing his daily

business and conducting with success the affairs of the "Steam Navigation Company," he was also engaged collecting facts for the improvement of the art itself, or for securing the safety of passengers. He devised simpler, cheaper, and safer forms of apparatus than those in general use. He investigated the influence of legal enactments for regulating steam navigation, and pointed out to legislatures and governments the inefficacy or inexpediency of such enactments, and suggested the true measures to be taken to promote the convenience and secure the safety of the public. He addressed a series of letters through the public prints to one of our prominent naval commanders, setting forth the adaptedness of steam as an agent of national defense. He responded to the call of the Secretary of the United States Treasury to point out the causes of steamboat explosions, and to suggest the means of safety. Happy would it be, if in all the great operations of the mechanical arts, the true spirit of the philosopher were so fully conjoined with the practical knowledge and skill of the engineer.

We turn now to another subject which engaged the attention of Mr. Redfield, and brought into exercise his remarkable sagacity and forecast. He was the first to place before the American people the plan of a system of railroads connecting the waters of the Hudson with those of the Mississippi. His pamphlet containing this project, issued in 1829, is a proud monument of his enlarged views, of his accurate knowledge of the topography of the vast country lying between these great rivers, of his extraordinary forecast, anticipating as he did the rapid settlement of the western states, the magic development of their agricultural and mineral wealth, and the consequent rapid growth of our great commercial metropolis. The route proposed is substantially that of the Erie railroad as far as this goes; but his views extended still further, and he marked out, with prophetic accuracy, the course of the railroads which would connect with the Atlantic states, the then infant states of Michigan, Indiana, and Illinois. These, he foresaw, would advance with incredible rapidity the settlement of those regions of unbounded fertility, and would divert no small portion of the trade from the Mississippi to the great metropolis of the east.

It must be borne in mind that railroads for general transportation were unknown in this country until 1826, when the project of constructing the Albany and Schenectady railroad was first entertained. As yet the advantages of railroads had not with us been practically demonstrated, and especially their advantages over canals were not generally understood or appreciated. At the moment when the Erie canal, having just been completed, was at the summit of its popularity, Mr. Redfield set forth in his pamphlet, under nineteen distinct heads, the great

superiority of railroads to canals, advantages which, although then contemplated only in theory, have been fully established by subsequent experience. He had even anticipated that after the construction of the proposed great trunk railway connecting the Hudson and the Mississippi, many lateral railways and canals would be built, which would bind in one vast net-work the whole great west to the Atlantic states. "This great plateau (says he) will indeed one day be intersected by thousands of miles of railroad communications; and so rapid will be the increase of its population and resources, that many persons now living will probably see most or all of this accomplished." How truly has this remarkable prediction, uttered in 1829, when there was not a foot of railroad in all the country under review, been fulfilled!

The motives which impelled Mr. Redfield to spread this subject before the American people at that early day, when railroads were scarcely known in this country, were purely patriotic. He had no private interests to subserve in the proposed enterprize, and the whole expense of preparing and publishing two editions of the pamphlet embodying these enlarged and prophetic views, was defrayed from his own limited resources.

In 1832, Mr. Redfield again issued a pamphlet, setting forth the importance of a railway leading from New York to Albany. This was no less evincive than the other of his public spirit and disinterestedness, since his private interests lay in the steamboat navigation of the Hudson, to which the proposed railway would be a powerful rival.

But we turn from these noble enterprises in which the philosopher and the engineer were happily united in the same individual, to the consideration of the great subject which, from this time, formed the leading object of his life, namely, to perfect his *theory of storms*. Nor do we turn away from great practical subjects to such as are merely speculative. The lives and property which Redfield's disinterested labors in behalf of steam navigation contributed to save, would, we believe, be of small amount compared with the sailors and ships which the rules founded on his theory of storms, when fully applied to practice, will save from shipwreck.

We have already seen that the attention of Mr. Redfield was first drawn to the subject of storms in the year 1821, by examining the position of trees prostrated by the great September gale, which passed over Connecticut and the western part of Massachusetts that year. Although he had never lost sight of the theory of storms, yet the multifarious business concerns which engrossed the greater part of his time for a number of years afterwards, prevented his bringing it distinctly before the public until the year 1831. I chanced at that period to meet him for

the first time on board a steamboat on the way from New York to New Haven. A stranger accosted me, and modestly asked leave to make a few inquiries respecting some observations I had recently published in the American Journal of Science on the subject of Hailstorms. I was soon made sensible that the humble inquirer was himself a proficient in meteorology. In the course of the conversation, he incidentally brought out his theory of the laws of our Atlantic gales, at the same time stating the leading facts on which his conclusions were founded. This doctrine was quite new to me, but it impressed me so favorably, that I urged him to communicate it to the world through the medium of the American Journal of Science. He manifested much diffidence at appearing as an author before the scientific world, professing only to be a practical man little versed in scientific discussions, and unaccustomed to write for the press. At length, however, he said he would commit his thoughts to paper, and send them to me, on condition that I would revise the manuscript and superintend the press. Accordingly, I soon received the first of a long series of articles on the laws of storms, and hastened to procure its insertion in the Journal of Science. Some few of the statements made in this earliest development of his theory, he afterwards found reason for modifying; but the great features of that theory appear there in bold relief. Three years afterwards he published, in the 25th volume of the same journal, an elaborate article on the Hurricanes of the West Indies, in the course of which he gives a full synopsis of the leading points of his doctrine, as matured by a more extended analysis of the phenomena of storms than he had made when he published his first essay.

Possibly some of our readers may not have fully acquainted themselves with Redfield's theory of storms, and would desire to be informed of its leading principles. I understand this theory to be substantially as follows:

That all violent gales or hurricanes are great *whirlwinds*, in which the wind blows in circuits around an axis either vertical or inclined; that the winds do not move in horizontal circles, as the usual form of his diagrams would seem to indicate, but rather in spirals towards the axis, a descending spiral movement externally and ascending internally.

That the *direction of revolution* is always uniform, being from right to left, or against the sun, on the north side of the equator, and from left to right, or with the sun, on the south side.

That the *velocity of rotation* increases from the margin towards the center of the storm.

That the whole body of air subjected to this spiral rotation is, at the same time, *moving forward* in a path, at a variable rate, but always with a velocity much less than its velocity of rota-

tion, being at the minimum, hitherto observed, as low as four miles, and at the maximum forty-three miles, but more commonly about thirty miles per hour, while the motion of rotation may be not less than from one hundred to three hundred miles per hour.

That in storms of a particular region, as the gales of the Atlantic, or the typhoons of the China seas, *great uniformity exists in regard to the path pursued,* those of the Atlantic, for example, usually issuing from the equatorial regions eastward of the West India islands, pursuing, at first, a course towards the northwest as far as the latitude of 30°, and then gradually wheeling to the northeast and following a path nearly parallel to the American coast, to the east of Newfoundland, until they are lost in mid-ocean, the entire path when delineated resembling a parabolic curve whose apex is near the latitude of 30°.

That their *dimensions* are sometimes very great, being not less than 1000 miles in diameter, while their path over the ocean can sometimes be traced for 3000 miles.

That the *barometer,* at any given place, falls with increasing rapidity as the center of the whirlwind approaches, but rises at a corresponding rate after the center has passed by; and finally,

That the phenomena are more uniform in large than in small storms, and more uniform on the ocean than on the land.

These laws Mr. Redfield claims as so many *facts* independently of all hypothesis; as facts deduced from the most rigorous induction, which will ever hold true, whatever views may be entertained respecting the origin or causes of storms.

The *method* adopted by the author of this theory, in all his inquiries,—the method which first led him to the discovery of the whirlwind character of storms, and afterwards fully confirmed the doctrine,—was first to collect and then to collate as many records as possible of vessels that had been caught in the storm, in various parts of the ocean. The most laborious and profound investigation of this nature of which he has left us an example, is in the case of the Cuba hurricane of October, 1844. First, he examined all accessible marine reports of vessels that had arrived in port after encountering the storm; secondly, he inspected the log-books of all such vessels, as far as was practicable, and carefully transcribed their records; and, thirdly, by an extended correspondence, he obtained a great number of written statements from shipmasters, who of all men would be the most accurate and vigilant observers. The different independent accounts obtained from these various sources amounted to no less than one hundred and sixty-four, all of which were reduced to the form of tables, containing the latitude and longitude of each vessel or place at the time of observation; the exact date and duration of the gale; the successive directions of the storm-

wind; the state of the barometer; and, finally, every additional particular that was deemed of the least importance in determining the peculiar characteristics of the storm. With these data before him, he spread out a marine chart, and having noted on it the position of each vessel and place with the direction and force of the wind, the plot itself proclaimed to the eye the whirlwind character of the storm; and the comparison of dates, and corresponding courses of the winds, and respective states of the barometer, showed the dimensions of the storm, its rotary and progressive velocities, its duration at any given place, and its various degrees of violence at different distances from the center. In the character of the researches before us, conducted as they were, not in the shades of philosophic retirement and learned leisure, but in hours redeemed from the pressing avocations of an onerous and responsible business, or borrowed from the season allotted to sleep, we trace qualities of mind that belong only to the true philosopher.

The benevolent and practical mind of Redfield had no sooner established the laws of storms, than it commenced the inquiry, what rules may be derived from it, to promote the safety of the immense amount of human life and of property that are afloat on the ocean, and exposed continually to the dangers of shipwreck; in this imitating our Franklin, who as soon as he had discovered the identity of lightning with the electricity of our machines, hastened to the inquiry, How may we so apply our knowledge of the laws of electricity as to disarm the thunderbolt of its terrors? We might pursue the comparison and say, that as every building saved from the ravages of lightning by the conducting rod, is a token both of the sagacity and the benevolence of Franklin, so every vessel saved from the horrors of shipwreck by rules derived from these laws of storms, is a witness to the sagacity and benevolence of Redfield. Other writers on the laws of storms, especially Reid and Piddington, have lent important aid in establishing rules for navigators, until it is now easy for the mariner by the direction in which the gale strikes his ship, to determine his position in the storm, and the course he must steer in order to escape from its fury. Nor are testimonies wanting of the successful application of these rules. The most accomplished navigators (we might instance particularly Commodores Rodgers and Perry, and Commander Glynn, of the U. S. Navy) have testified that within their knowledge and in some cases within their own observation, many ships have owed their deliverance from the perils of shipwreck to a faithful observance of the rules derived from Redfield's theory of storms. In no department perhaps of the studies of nature have mankind been more surprised to find things governed by fixed laws than in the case of the winds. It is now rendered in the highest

degree probable, that every breeze that blows is a part of some great system of aerial circulation and helps to fulfill some grand design. "Inconstant as the winds" has long been a favorite expression to denote the absence of all uniformity or approach to fixed rules; but the researches of the meteorologists of our times, force on us the conclusion that the winds, even in the violent forms of hurricanes and tornadoes, are governed by laws hardly less determinate than those which control the movements of the planets.

It has been often noticed in the history of science and the arts, that great discoveries and inventions spring forth simultaneously from different independent sources. Thus the discovery of oxygen gas, the greatest single discovery in chemistry, was made almost at the same moment by Priestley in England and Scheele in Sweden; and the method of fluxions, or the infinitesimal calculus, was invented at nearly the same time by Newton and Leibnitz. Such discoveries and inventions are the true resultant of innumerable forces, which at that moment, and never until then since the origin of time, all conspired. It is remarkable that the idea that great storms are progressive whirlwinds was, for the first time, embraced nearly at the same instant by Redfield and Dové, although the conclusion was arrived at by totally different methods of investigation. Mr. Redfield says in a note to his paper on the Cuba hurricane, published in 1846, that it was not until seven years after the publication of his theory of the rotary and progressive character of storms, that he became acquainted with the suggestions and opinions of Col. Capper, and with the particular views and elucidations published by Professor Dové in his paper on Barometric Minima found in Poggendorff's Annalen for 1828. To all who were personally acquainted with Redfield, it would be quite unnecessary to adduce any other evidence than his simple declaration, of the perfectly original and independent character of his theory of the laws of storms. But we might refer to the circumstances under which it was conceived, when he was far removed from all libraries, and all intercourse with the scientific world; and as respects Dové, in particular, whose essay was communicated to the public in 1828, it may be said, that at that period there was scarcely a copy of Poggendorff's Annalen, in which Dové's essay appeared, in the United States; and being in the German language, nothing could be more improbable than that its contents were then known to Redfield. In 1838, our friend found to his great joy a most able ally in Col. Reid of the Royal English Engineers, then stationed in the island of Barbadoes. The earliest inquiries of Col. Reid were based on a violent hurricane, which occurred in that island in the year 1831. Searching for accounts of previous storms, he met with nothing satisfactory until he fell in with

Redfield's earliest paper respecting the September gale of 1821, published in the American Journal of Science. With the view of testing Redfield's doctrines, he submitted to the closest scrutiny the records which the Barbadoes storm had left of its ravages,—an investigation which ended in a perfect conviction that this storm was a progressive whirlwind. A friendly correspondence was shortly afterwards opened between these two congenial spirits, which resulted in an intimacy unbroken except by the hand of death. Commodore Perry, in the recent Report of his Japan Expedition, thus expresses himself in an introductory note to Mr. Redfield's Essay (the latest of his published works) on the Cyclones of the Pacific, addressed to Commodore Perry, and forming a part of his volume. "It was my good fortune (says the Commodore) to enjoy, for many years, the friendly acquaintance of one as remarkable for modesty and unassuming pretensions, as for laborious observation and inquiry after knowledge. To him and to Gen. Reid of the Royal Engineers of England (now governor of Malta) are navigators mainly indebted for the discovery of a law which has already contributed, and will continue to contribute, greatly to the safety of vessels traversing the ocean. It is true that subsequent writers have furnished additional information on this subject; but to Redfield and Reid should be ascribed the credit of the original discovery of this undeniable law of nature and its application to useful purposes; and there can be nothing more beautiful, as illustrative of the character of these two men, than the fact, well known to myself, that notwithstanding their simultaneous observations and discoveries, in different parts of the world, neither claimed the slightest merit over the other, but each strove to give to his co-worker in research the meed of superior success in the great object of their joint labors; and thus, without ever meeting, a strong friendship was formed between them, growing out of congenial aspirations for an honorable fame, and mutual admiration of the generous and enlightened views exhibited by each other; and this ennobling feeling was kept alive to the last by friendly correspondence."

The idea of whirlwinds is indeed much older than Redfield or Reid, being as old as the writings of the psalmist and the prophets; and we safely admit further, that the doctrine of ocean gales being sometimes of a rotary character, had been hinted at by several writers, as hints of such a principle as gravitation had long preceded the investigations of Newton; but the honor of having established, on satisfactory evidence, the rotary and progressive character of ocean storms, and determining their modes of action or laws, it is due alike to the memory of the departed, and to our country's fame, to claim for WILLIAM C. REDFIELD.

Back of the laws that govern these ocean gales, as first determined by Redfield and confirmed by Dové, Reid, Piddington,

Thom, and other well known writers, lies a more profound inquiry, How are these laws themselves to be accounted for? What sets the storm in motion, and gives it the whirlwind character, and at the same time carries it forward, and in so definite a path? What makes it revolve always from right to left on the north side of the equator, and from left to right on the south side? Why does its violence increase towards the centre of the storm, and why is its force there so tremendous? Laws, it must be remembered are facts, and merely express the modes in which nature acts: they are themselves phenomena to be accounted for. To which of the ultimate causes of physical phenomena is their origin, in the present case, to be traced? Is it heat? Is it electricity? Is it gravity? Is it connected in some way with the grand system of planetary motion? Questions of this kind were pressed on Mr. Redfield from various sources by those who assailed his theory. At first he declined any attempts at their solution. He claimed that the whirlwind character of storms, and the laws which he had assigned to them, are matters of fact, as established not only by himself, but also by Reid, Milne, Dové, and Piddington; that never having attempted to establish a theory of winds, nor the origin or first cause of storms, he had no occasion to go into these inquiries, but had long held the proper inquiry to be, *What are storms?* not *How are storms produced?* He however incidentally, at different times, indicated his opinions on the ultimate causes of storms. Electricity, Redfield entirely rejected as an agent in the production of winds and storms, considering its presence and development rather as a consequence than as a cause of atmospheric changes. To heat he assigned only a limited and local effect, denying its agency in producing either the great and established movements of the atmosphere, or the extraordinary commotions which constituted the chief objects of his study, hurricanes and tempests. But he considered what he called the "dynamics of the atmosphere," as connected with and resulting from the diurnal and annual motions of the earth. While, from the first, I have heartily embraced Redfield's doctrine that ocean gales are progressive whirlwinds, and have further fully believed that he had established these laws or modes of action on an impregnable basis, a regard to truth and candor obliges me to say, that I have never been a convert to his views respecting the ultimate causes of storms, especially so far as he assigned for these causes what he denominates the "diurnal and orbitual motions of the earth," but his notions on this point have always appeared to me vague and unsatisfactory. Nor, while I have been impressed with the belief that *heat* is, in general, by far the most influential of all natural agents in destroying the equilibrium of the atmosphere, and of causing its motions, both in established currents, as the trade

winds and the monsoons, and in its violent commotions, as in hurricanes and tornadoes, yet I am compelled to think that but little progress has yet been made in determining its *modus operandi*, or in tracing the *connection* between changes of temperature and the actual phenomena of winds and storms:—why, for example, the Atlantic gales originate where they do, in the tropical regions—why they first pursue a path to the northwest as far as the latitude of 30°, and then gracefully wheel in parabolic curves towards the northeast, and pursue this course for the remainder of their way—why they revolve on their axes and always in one direction—whence they acquire so tremendous a force, especially towards the central parts—why the barometer is so low in the center and so high in the margin of the storm. These and various other points connected with the whirlwind character of storms, seem to me to have met hitherto with but a partial and doubtful solution. The laws constitute the true theory of storms: the rest is yet hypothesis.

Various writers have severally displayed great ingenuity and profound knowledge of atmospheric phenomena, in their endeavors to solve these problems, but with respect to the causes which lie back of the laws of storms, we still remain to a great degree in ignorance. Each of the combatants appears to me to be more successful in showing the insufficiency of the other's views than in establishing his own. With respect to him who is more particularly the subject of my remarks, whose logical powers I have always admired, I have almost regretted that he did not adhere to the ground he originally took, namely, that he had not undertaken to explain the reason *why* the winds blow, but only to show *how* they blow. So far was matter of fact: all beyond was hypothesis. His facts are impregnable: his hypothesis doubtful. The conclusions derived legitimately from these facts constitute the laws of storms; and being, as we believe, like the other laws of nature immutable, the name indissolubly associated with their discovery, acquires a fame alike imperishable. Redfield might therefore have safely stopped where Newton stopped. "Newton (says one of his biographers) stopped short at the last fact which he could discover in the solar system—that all bodies were deflected to all other bodies, according to certain regulations of distance and quantity of matter. When told that he had done nothing in philosophy; that he had discovered no cause; and that, to merit any praise, he must show how this deflection was produced; he said, he knew no more than he had told them; that he saw nothing causing this deflection; and was contented with having described it so exactly, that a good mathematician could now make tables of the planetary motions, as accurate as he pleased, and hoped in a few years to have every purpose of navigation and philosophical curiosity completely answered."

Various other contributions to science of our departed friend must, for want of space, be passed by with hardly a notice. Such are his published meteorological essays*—his reports of meteorological observations, which contain many original hints of much value—his paper on the currents of the Atlantic—and his researches in geology, which occupied much of his attention during the latter years of his life—all of which speak the skilful observer, the judicious philosopher, the lover of science, the lover of his country and of his kind. Three distinguishing marks of the true philosopher met in William C. Redfield—originality to devise new things; patience to investigate; and logical powers to draw the proper conclusions. The impress of his originality he left, in early life, upon the village where he resided; he afterwards imprinted it still deeper on his professional business, as naval engineer; and most of all on his scientific labors, his observations, and his theories. "Patient thought" was the motto of Newton, and in this attribute, Redfield was eminently distinguished. In collecting facts bearing upon his main purpose, and in submitting them to severe and long continued comparison, he has illustrated this quality in its highest forms, as his laborious investigation of the phenomena of hundreds of storms, most fully evince. Originality to invent without patience to investigate, leads to hasty and wild speculations; but united they lay the deep foundations for a severe logic. His powers of reasoning have always appeared to me to be of high order, and he has been fitly characterized by another eminent writer† on the laws of storms, as the "clear-headed" Redfield. Opinions which he had thus formed, after an extensive and patient investigation of the facts, and a severe process of reasoning, he held with great tenacity. But though firm, he was not obstinate. *Obstinacy* we define to be an unyielding adherence to our opinions because we have adopted them: *firmness*, a similar adherence to our opinions, because we believe them to be right.

Few men have given more signal proofs of an original inherent love of knowledge. Whether we contemplate the apprentice-boy after the toils of the day, seeking for knowledge by the dim light of an open fire; or the father of a young family, through dark scenes of domestic affliction and mournful bereavements, still adding largely year by year to his intellectual stores; or the man of business in the whirl of the great metropolis, loaded with onerous and responsible cares, giving every interval of leisure, and the seasons chiefly employed in pleasure or repose to the study of the laws of nature; or if permitted, as has been my privilege, to be a guest at the house fitted up to be the retreat of his old age, we see the library, the collections of natural his-

* Originally prepared for Blunt's Coast Pilot.

† Reid.

tory, the many sources of high mental enjoyment, which in the period gained at last of ease and affluence, distinguish the different apartments of his dwelling; or finally whether we call to mind the ever increasing interest with which he attended the meetings of the American Association for the Advancement of Science, and the delight which he experienced in the society of learned men, we observe in all, a mind in love with truth, ever searching and ever expanding. In society he was courteous, sincere, upright, and benevolent; in his family, tender, affectionate, wise in council and pure in example; in all his walk and conversation, and especially in the church of God, a devout and humble christian.

As the evening of life was passing thus serenely, it hastened to a peaceful close. Mr. Redfield's health had been generally good during his later years, and had seemed particularly so in the early part of the winter which proved his last. On the first of January, he made his usual calls on his friends, and the cheerfulness and vivacity of his manners and healthful expression, were never more remarkable. Near the last of January he was seized with alarming symptoms, which indicated effusion in the chest. His disease made rapid and sure progress. The last book which had engaged his attention previous to his illness was Dr. Kane's recent Narrative of his Arctic Expedition, and his own feverish dreams, during the earlier nights of his sickness, were confusedly identified with the toils, the difficulties, and the sufferings of that heroic commander and his brave companions. With a general tendency to delirium were mingled intervals of calmness, and throughout his illness his countenance would light up with the smile of affection, as he recognized the relations and friends around him. From the first he entertained but slight hopes of recovery; but as the crisis drew near, his mind was at peace, and in calm resignation to the will of his Maker, and in the full exercise of christian faith, he gently breathed his last.

List of the published papers of the late WM. C. REDFIELD.

1. Sketch of the Geographical Route of a great Railway, by which it is proposed to connect the Canals and navigable waters of the States of New York, Pennsylvania, Ohio, Indiana, Illinois, Missouri, and the Michigan, North West, and Missouri Territories, opening thereby a free communication at all seasons of the year between the Atlantic States and the great valley of the Mississippi. Pamphlet, published by G. & C. & H. Carvill, New York, 1829.

A second edition of the above, with an appendix of much additional matter, was published by the same house in 1830.

2. Remarks on the prevailing storms of the Atlantic Coast of the North American States. American Journal of Science, vol. xx, p. 17. April, 1831.

3. List of Steamboat Explosions which have occurred in the United States, with some remarks on the same. Am. Journ. Sci., xx, 336. July, 1831.

4. On the supposed collapse of Steam-boilers, and the means of preventing explosions. Amer. Journ. Sci., xxi, 190. Oct. 1831.

5. Note on the Hurricane of August, 1831. N. Y. Journal of Commerce, Sept. 28, 1831. Copied in Am. Jour. Sci., xxi, 191, Oct. 1831.

6. Facts and Suggestions relating to the New York and Albany Railroad. Pamphlet, New York, 1832.

7. Notice of American Steamboats. Amer. Journ. Sci., xxiii, 311, Jan. 1833.

8. Observations on the Hurricanes and Storms of the West Indies and the Coast of the United States. Blunt's American Coast Pilot, 12th edition, p. 626, July, 1833, and in subsequent editions; also in Amer. Jour. Sci., xxv, 114, Oct. 1833; translated in Bibl. Univ. de Genève, i, 412, 1834.

An Abstract of the above under the title of "Monitions and Instructions for vessels navigating on the western side of the Atlantic" is found in Purdy's Memoir upon the Atlantic Ocean, 10th edition, 1853, p. 303.

9. Summary Statements of some of the leading Facts in Meteorology. Amer. Journ. Sci., xxv, 122, Oct. 1833. Originally prepared for the American edition of "Million of Facts," published in New York, 1833. It constitutes the new matter under the head of "Atmospheric and Aerial Phenomena" in that work. Some copies have been circulated in pamphlet form under the title of "Facts in Meteorology."

10. Report of the Board of Examiners appointed by the Connecticut River Steam Boat Company to enquire into the Causes of the Explosion of the Steam Boat New England. Published by the Steam Boat Company. Dec. 1833.

11. Synopsis of a Meteorological Journal kept in the city of New York during the years 1833 and 1834. Report of the Regents of the Univ. of State of N. Y. for 1835. p. 183; Amer. Journ. Sci., xxviii, 154, April, 1835.

12. On the Evidence of certain Phenomena in Tides and Meteorology, (In reply to Notes of an Observer [Mr. Espy]). Journ. Franklin Institute, Phil., xv, 372, June, 1835; Amer. Journ. Sci., xxviii, 310, July, 1835.

13. On the Gales and Hurricanes of the Western Atlantic. London Nautical Magazine, v, 199, April, 1836; U. S. Naval Magazine, i, 301, July, 1836; Amer. Journ. Sci., xxxi, 115, Oct. 1836.

14. On the Whirlwind character of certain Storms. (In reply to Mr. Espy's No. 3, in a series of Meteorological Essays in Journ. of Franklin Institute). Journ. Franklin Institute, xix, 112, Feb. 1837.

15. Remarks on the supposed connection of the Gulf Stream with opposite currents. U. S. Naval Magazine, ii, 243, May, 1837; Journ. Franklin Institute, xix, 384, May, 1837; Blunt's Amer. Coast Pilot, 13th edition, p. 666, June, 1837, and in subsequent editions; American Journ. Sci., xxxii, 349, July, 1837.

16. Synopsis of a Meteorological Journal kept in the City of New York, during the years 1835 and 1836; reported to the Albany Institute. Report of Regents of Univ. of State of N. Y. for 1837, p. 214, March, 1837.

17. Cursory Remarks and Suggestions on various topics in Meteorology, by an Amateur Observer. Blunt's Amer. Coast Pilot, 13th ed., p. 689, and subsequent editions, June, 1837.

The same under the title of "Meteorological Sketches, by an Observer." Amer. Jour. Sci., xxxiii, 50, 261, Oct. 1837 and Jan. 1838; U. S. Naval Magazine, ii, 392, 1837.

18. Some account of two visits to the Mountains in Essex Co., N. York, in the years 1836 and 1837; with a sketch of the northern sources of the Hudson. Am. Journ. Sci., xxxiii, Jan. 1838; Family Magazine, N. Y., v, 345.

19. Notice of Fossil Fishes in Virginia. Amer. Jour. Sci., xxxiv, 201, April, 1838.

20. Synopsis of Meteorological Journal kept in the City of New York for the year 1837, including the average results of the last five years. Report of Regents, 1838 or 1839; Am. Journ. Sci., xxxiv, 373, July, 1838.

21. Letter to Lieut. James Hoskin, R. N., Commander of the Steamship Great Western, on the speed of Steamboats. N. Y. Journal of Commerce, Aug. 8, 1838; London Nautical Magazine, 1838, p. 673; Jameson's Journal, Feb., April, 1838.

22. On the courses of Hurricanes, with notices of the Typhoons of the China Sea, and other Storms. Amer. Journ. Sci., xxxv, 201, Jan. 1839; London Nautical Magazine, 1839, p. 1, Jan. 1839.

23. Genealogy of the Redfield Family in the United States. Printed for private circulation, March, 1839.

24. Some Account of violent columnar Whirlwinds which appear to have resulted from the action of large circular Fires, with Remarks on the same. (Read before the Connecticut Academy of Arts and Sciences, Jan. 22, 1839.) Amer. Jour. Sci., xxxvi, 50, April, 1839; Bibliothèque Univ. de Genève, 1840, xxviii, 432.

25. Additional Facts relating to the Raleigh's Typhoon of August 5th and 6th, 1835, in the China Sea. Amer. Journ. Sci., xxxvi, 59, April, 1839; London Nautical Magazine, 1839, p. 461.

26. Letter to the Secretary of Treasury of U. S. on Steamboat Explosions and their preventions, dated Dec. 26, 1838. Congressional Document, 25th Congress, 3d Session, House of Reps., Doc. No. 21, 1839; London Nautical Magazine, 1839, p. 617, 681 and 813.

27. Remarks on Mr. Espy's Theory of Centripetal Storms, especially his positions relative to the storm of Sept. 3d, 1821, with some notice of his examinations of other storms. Journ. Franklin Institute, xxiii, 323, 363, May and June, 1839.

28. Further notice of Mr. Espy's examination of Storms. Journ. Franklin Inst., xxiv, 1, July, 1839.

29. Account of the Circular Storm of Dec. 1839. Journ. of Commerce, Jan. 6, 1840; London Nautical Magazine, 1840, 424.

30. On the Causes of Steamboat Accidents and the practical means of preventing their occurrence. (Communicated by request through Capt. J. W. Pringle, R. E., to the Commissioners on that subject appointed by the English Government. Received too late for the Report of the Commissioners), but published here in "Appendix to a Memorial of Proprietors and Managers of American Steam-vessels on the impolicy and injustice of certain enactments contained in the law relating to steamboats." Jan. 1840.

31. Abstracts of Meteorological Observations made at St. John's, Newfoundland, and at Canton in China, with some notice of the Half-yearly Inequalities of Atmospheric Distribution which appear in these observations. Am. Jour. Sci., xxxviii, 265, April, 1840.

32. Synopsis of a Meteorological Journal kept in the City of New York for the years 1838 and 1839, including also the mean results of the last seven years. Report of the Regents of the University of State of N. Y., 1840; Am. Journ. Sci., xxxviii, 323, April, 1840.

33. Short Notices of American Fossil Fishes. (Read before the Yale Natural History Society.) Am. Jour. Sci., xli, 24, July, 1841.

34. Five Letters to Commodore Perry on the means of National Defence. N. Y. Journal of Commerce, July, 1841.

35. Remarks relating to the Tornado which visited New Brunswick in the State of New Jersey on the 19th June, 1835, with a plan and schedule of the prostrations observed on a section of its track. London, Ed. and Dub. Phil. Mag. and Jour. Sci., xviii, 20, Jan. 1841; Am. Jour. Sci., xli, 69, July, 1841; Jour. Frankl. Inst., xxviii (or 3d series ii), 40, July, 1841.

36. Observations on the Storm of Dec. 15th, 1839, with a Map showing the direction of the wind at noon of that day, as observed at various places. (Read before the Am. Phil. Soc., Jan. 15, 1841.) Lond., Ed. and Dubl. Phil. Mag., xvii, 17, July, 1841; Am. Jour. Sci., xlii, 112, Jan. 1842; Trans. Am. Phil. Soc., viii, new series, 77, 1843.

37. Extract of a letter to Sir J. F. W. Herschel. Report of 10th meeting of British Assoc. for Adv. of Science, 1841, p. 40, 1841.

38. Reply to Dr. Hare's Objections to the Whirlwind Theory of Storms. Am. Jour. Sci., xlii, 299, April, 1842; Lond., Ed. and Dubl. Phil. Mag., xx, 353, May, 1842.

39. Reply to Dr. Hare's Further Objections relating to Whirlwind Storms, with some evidence of the whirling action of the Providence Tornado of August, 1838. Am. Jour. Sci., xliii, 250, Oct. 1842; latter portion of the article also in Amer. Repertory of Arts and Sci., iii, 81, March, 1841; also in Jour. Frank. Inst., new series, iv, 252, Oct. 1842; first portion of article also in Lond., Ed. and Dubl. Phil. Mag., xxi, Suppl. 481, Dec. 1842; latter portion of article also in same, xxii, 38, Jan. 1843.

40. Notice of newly discovered Fish-beds and a Fossil Foot-mark in the Red Sandstone Formation of New Jersey. Am. Jour. Sci., xliv, 134, Jan. 1843.

41. Notice of Dr. Hare's Strictures on Prof. Dove's Essay on the Law of Storms. Am. Jour. Sci., xliv, 384, April, 1843; Jour Franklin Inst. for 1844, p. 384.

42. Remarks on Tides and the prevailing Currents of the Ocean and Atmosphere. (Read before Amer. Phil. Soc. at its centennial meeting, May 27th, 1843.) Am. Jour. Sci., xlv, 393, Oct. 1843; London Nautical Magazine, 1843, 655.

43. On the drift Ice and Currents of the North Atlantic, with a Chart showing the observed positions of the Ice at various times. Am. Jour. Sci., xlviii, 373. April, 1845; London Nautical Magazine, 1845, p. 298, 353; Blunt's Memoir on the Dangers and Ice of the North Atlantic.

44. Letters to the U. S. Board of Navy Commissioners, being replies to their circular asking information relative to Steam Navigation applicable to government purposes. Jour. Frankl. Inst., new series, x, 361, and xii, 1, June, July and Aug. 1846.

45. On three several Hurricanes of the American Seas, and their relation to the Northers so called of the Gulf of Mexico and the Bay of Honduras, with Charts illustrating the same. Am. Jour. Sci., new series, i, 1, 153, 333, ii, 162, 311, Jan., March, May, Sept. and Nov. 1846.

46. Effects of the Earth's Rotation upon Falling Bodies, and upon the Atmosphere. Am. Jour. Sci., new series, iii, 283, March, 1847.

47. Remarks on a Letter from R. N. Shufeldt of U. S. Ship Marion to E. and G. W. Blunt, relative to a Hurricane encountered by Ship Marion, Sept. 1848. London Nautical Magazine, 1849, p. 39.

48. On some Fossil Remains from Broome Co., N. Y. Proceedings of Am. Assoc. for Adv. of Science, 2d Meeting, 1849, (at Cambridge,) p. 255.

49. The Law of Storms and its penalties for neglects. N. Y. Jour. of Commerce, June 19, 1850; London Nautical Magazine, 1850, p. 470; Bermuda Royal Gazette, July 16, 1850; N. Y. Courier and Enquirer.

50. On the apparent necessity of revising the received systems of Dynamical Meteorology. Proc. Am. Assoc. for Adv. of Sci., 4th Meeting, (New Haven,) p. 366, 1850.

51. On the Post-Permian Date of the Red Sandstone Rocks of New Jersey and the Connecticut Valley, as shown by their Fossil Remains. Proc. Am. Assoc. for Adv. of Science, 5th Meeting, (Cincinnati,) p. 45, May, 1851.

52. On the Fossil Rain-marks found in the Red Sandstone Rocks of New Jersey and the Connecticut Valley, and their authentic character. Proc. Am. Assoc. for Adv. of Science, 5th Meeting, (Cincinnati,) p. 72, May, 1851.

53. On the value of the Barometer in navigating the American Lakes. Proc. Am. Assoc. for Adv. of Science, 7th Meeting, (Cleveland,) p. 54, 1853; Wells's Annual of Scientific Discovery for 1854, p. 200.

54. A storm-track of Seven Thousand Miles. Annals of Science, Cleveland, vol. ii, p. 47, 1854.

55. Cape Verde and Hatteras Hurricane and other Storms, with a Hurricane Chart. Proc. Amer. Assoc. for Adv. Sci., 8th Meeting, (Washington,) p. 208, 1854. This paper and the preceding are abstracts of the following.

56. On the first Hurricane of September, 1853, in the Atlantic, with a Chart, and Notices of other Storms (Cape Verde and Hatteras Hurricane). Am. Jour. Sci., new series, xviii, 1, 176, July and Sept. 1854; London Nautical Mag., 1854, p. 466, 526, 597, Sept., Oct., Nov. 1854.

57. On the Storm of October 7th, 1854, near the coast of Japan, and the Conformity of its progression with other Cyclones. Proc. Am. Assoc. for Adv. of Sci., 9th Meeting, (Providence,) Aug. 1855, p. 183.

58. On the relations of the Fossil Fishes of the Sandstone of Connecticut and other Atlantic States to the Liassic and Oolitic Periods. Read before the American Assoc. for Advancement of Science, Albany, Aug. 26, 1856. Am. Jour. Sci., new series, xxii, 357, Nov. 1856; Proc. Am. Assoc. for Adv. of Sci., 10th Meeting, Albany, p. 180, 1857.

59. On the Spirality of Motion in Whirlwinds. Am. Jour. Sci., new series, xxiii, 23, Jan. 1857.

60. *Notes* attached to a Communication "On the Avoidance of the violent portions of Cyclones, with notices of a Typhoon at the Bonin Islands," (by John Rodgers, Commander U. S. N., and Anton Schönborn, Assist. Ast.) Am. Jour. Sci., new series, xxiii, 205, March, 1857.

61. On Pacific Cyclones. Am. Jour. Sci., xxiv, 21, July, 1857. An abstract of the following.

62. Observations in relation to the Cyclones of the Western Pacific, embraced in a Communication to Commodore Perry. Perry's Japan Expedition, ii, 335, July, 1857.

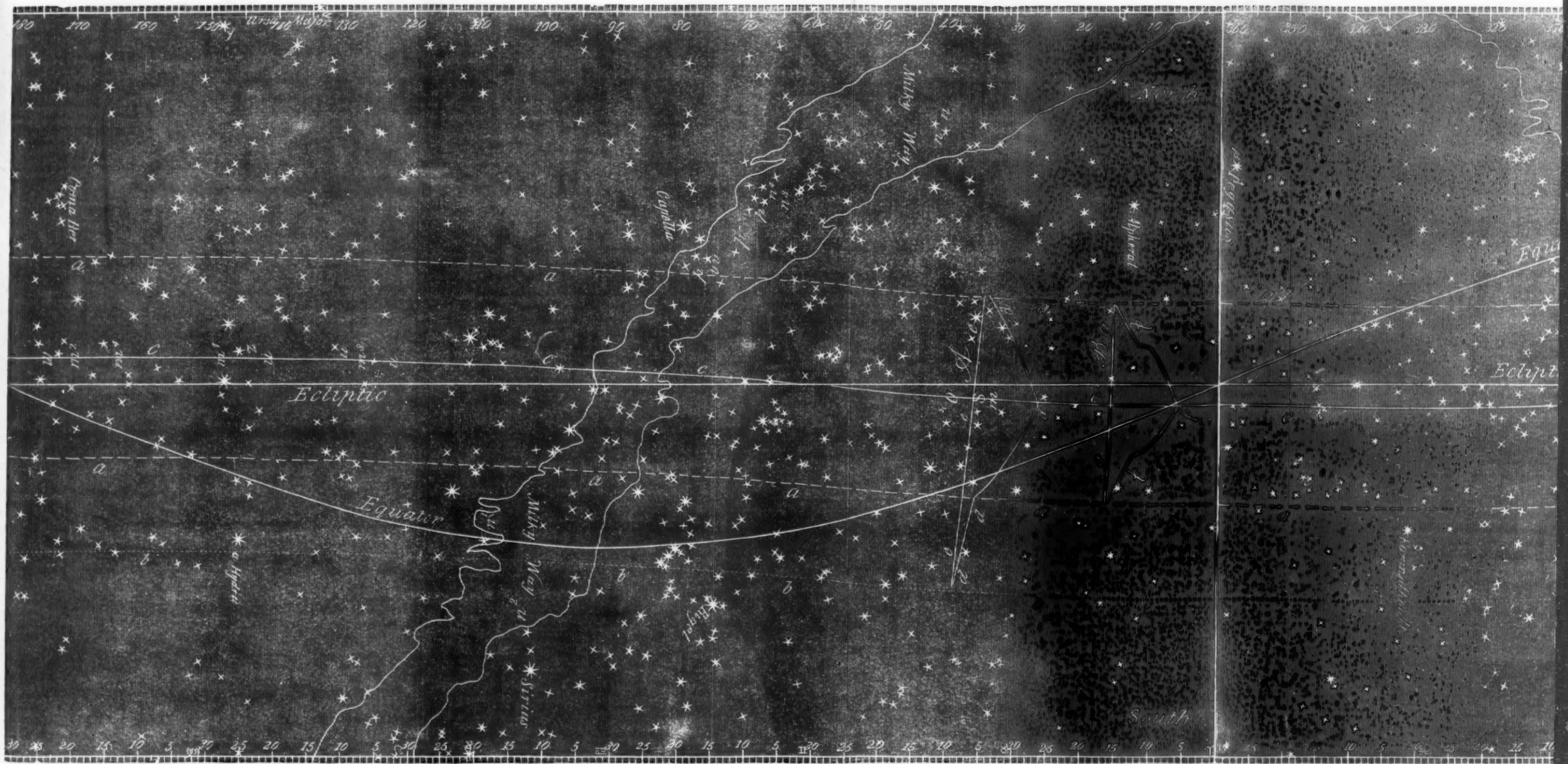
ART. XXVII.—*Observations on the Zodiacal Light at Quito, Ecuador, with deductions;* by Rev. GEORGE JONES, Chaplain U. S. Navy.*

THE deduction which I drew from observations made on the Zodiacal Light while engaged on the Japan cruise, namely, that there is a great nebulous ring having the earth for its centre, has been variously received; but I believe it is now beginning to be, among the astronomers of this country, an admitted fact. The book containing my detailed observations has gone abroad so recently that we have not yet had time to hear how the subject has been received in Europe. There have been some opinions advanced antagonistic to my conclusions (one of them an article in this Journal) to which I have made no replies: for I do not intend to be drawn into controversy, but to rely quietly on the facts of the case to do their work of conviction in the minds of thinking men.

In the Introduction to the 3d vol. of the Report on the Japan Expedition I intimated my intention to spend a year, if possible, at or near the equator; and to procure there additional data on this subject, of which I felt greatly the need. It is customary, when an officer has returned from a long cruise, to give him some time on "leave of absence:" which custom I pleaded with the Navy Department, when I had finished bringing out my volume of the Japan Report; and thus I obtained permission to be absent from the country, on my own business, for a year. My desire was to proceed to Quito, where, in its proximity to the equator and in the transparent atmosphere of its great altitude, I hoped to have peculiar advantages for my work. At the equator, an observer must have the ecliptic vertical to him, at some hour or other, every night throughout the year; and it must at all hours, have an angle with the horizon sufficiently high to show the Zodiacal Light to the very best advantage. My hope was to be able to ascertain exactly the position of this nebulous ring in regard to the ecliptic: I had a faint hope also,—a very slight one,—of being able to learn something about the nature of nebulous matter—which, in astronomical science, may now perhaps be considered the greatest question of the age.

Before starting on this expedition I prevailed on Mr. Edward C. Herrick of Yale College, who had previously had much experience on this subject, to agree to make observations in New Haven, on the 1st, 10th and 20th of each month, and to record them on star-charts left with him for that purpose, and, soon after, succeeded in enlisting in the same cause Professor Moesta, Superintendent of the National Observatory at Santiago, Chile;

* Contributed to this Journal by the special request of the Editors.


Ecliptic
Equator
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Milky Way
Milky Way
Capella
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Sirius
Rigel
Procyon
Hydra
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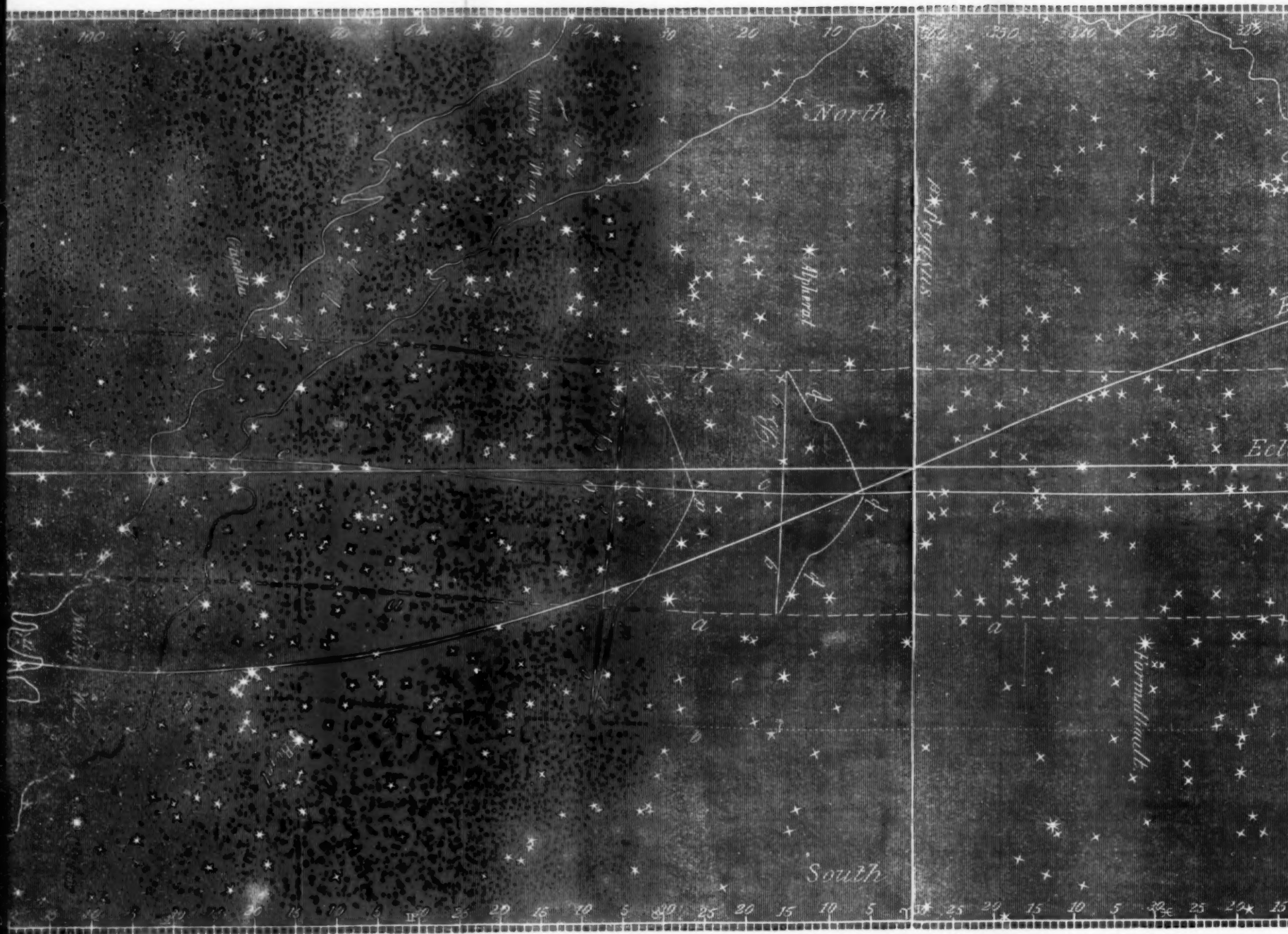
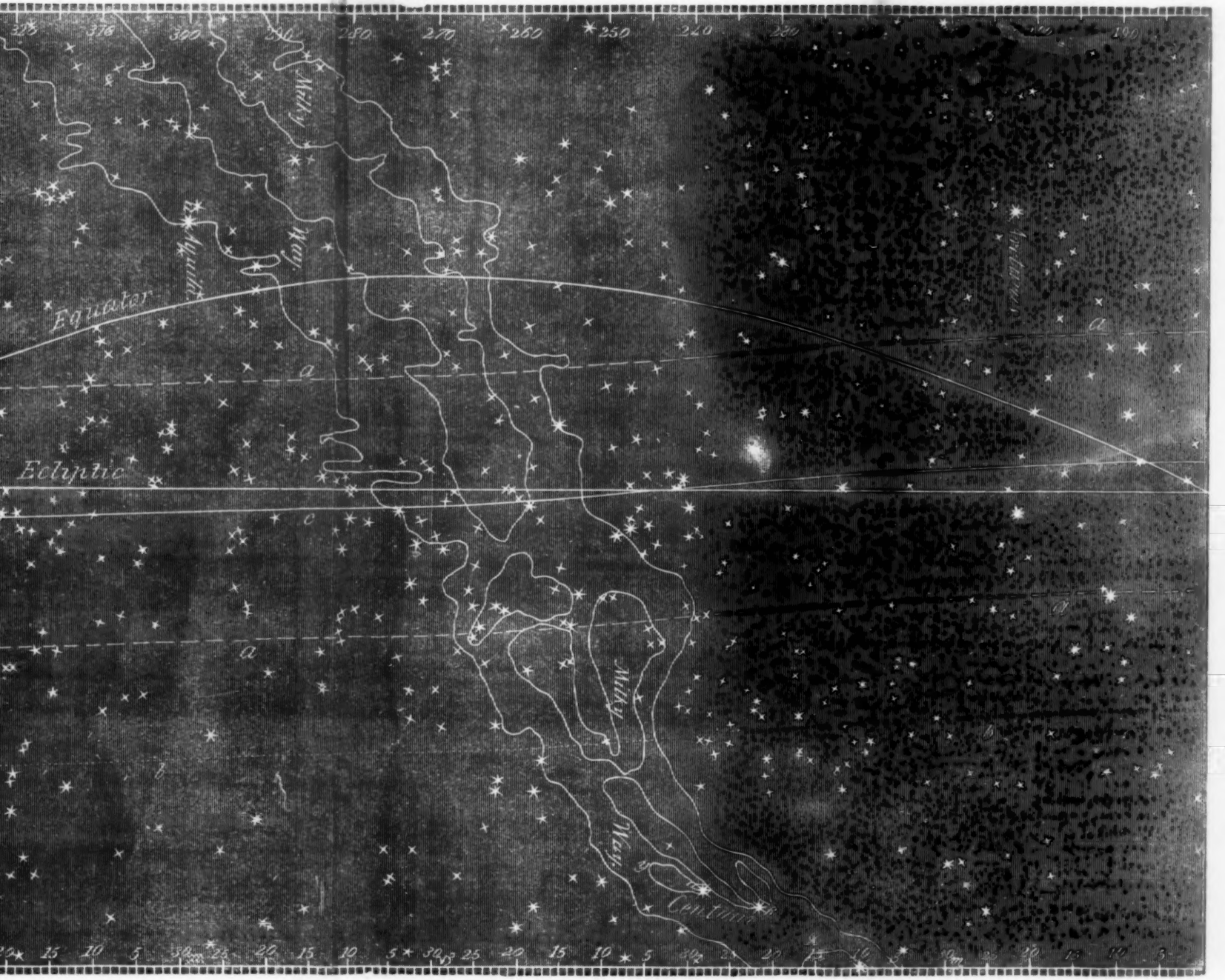
Eclipt
North
South


North
South
Milky Way
Capella
Aphekal
Pegasus
Formalhaut
Ecl


Milky Way
Equator
Ecliptic
Milky Way

Prof. M., who was furnished with charts, agreeing to make observations simultaneously with those of Mr. Herrick. Both of these gentlemen entered readily and cordially into the work: and I was able also, on the way to Ecuador, to join to our corps of observers a valuable coadjutor in Dr. Drayton, U. S. Navy, then attached to the sloop-of-war St. Mary's, stationed at Panama.

Quito is much troubled with clouds and rains. Nine months in the year are rainy; and only June, July and August are usually favored with clear skies: sometimes the clouds and rains continue through all the year. I was detained at Washington on duty till July: was then kept two weeks at Panama waiting for the English steamer to proceed south: a friend accompanying me was taken sick with the Panama fever and died on board the steamer: other passengers were dying when we arrived at Guyaquil; and we were ordered off without being permitted to land. Consequently I was not able to reach Quito till the close of August; from which time on, during all my stay of eight months on the Cordilleras, I had to contend unceasingly with the clouds. The work of observing was most laborious: for the clouds, in that region, come and go, without a minute's warning, forming suddenly in the sky without apparent cause, and in the same mysterious manner presently leaving it perfectly clear. Sometimes, while tracing the boundaries of the Zodiacal Light by a cluster of stars, I would find the cluster in a moment blotted out by some imperceptible agency, while all around it would be clear and bright: a few minutes' waiting would show it again in all its brightness; the vapor not having floated away, but having vanished as suddenly and as strangely as it had come. All this makes an observer's life there an anxious and watchful one. But when the sky is clear, it is a superbly glorious sight, such as I have never beheld in any other part of the world. The heavens are then fairly crowded with the smaller stars: and the Milky Way has a brightness and an apparent proximity wonderful to behold. It is to be lamented that there is not an observatory, with good telescopes, at that place. The government of Ecuador, unable to accomplish such a work itself, would be glad to see it done, and would furnish every facility that its means will allow.

I found there, that every thing distinctive in any celestial phenomenon, was brought out with a distinctiveness I had never before witnessed. Soon after reaching these altitudes, on going out, about midnight, to see whether I could find the Zodiacal Light over both horizons, as I had done when under peculiarly favorable circumstances in other parts of the world, I saw this light not only over the horizons but also *quite across the sky*. It formed a complete arch, passing near my zenith from the east to west; and was most distinctly and decidedly marked—looking as if a belt of thin white gauze were drawn across the heavens—

being clearly visible, but still not perceptibly dimming the stars beyond. My eye soon became accustomed to tracing it: and, from that time on, I never failed to see this luminous arch, at every hour of the night, when the moon or clouds did not prevent. I soon perceived that a cross-section of it would not present the same intensity of light quite across: but that it was brightest at its central part or central longitudinal line, dimming thence towards the edges or boundaries, which latter I sometimes found it difficult to make out. At the central part (running lengthwise) the line of strongest light was so well marked that I soon began to draw it on my charts, where I consider it fully reliable, showing both the inclination of this arch or ring to the ecliptic and the places of the nodes. As a proof of its clearness of marking among the stars, I may mention, that on the three different occasions, when other persons who had never before noticed the Zodiacal Light came to observe with me, and I said to them merely, "You see that luminous arch up in the sky:" "you see that, at its central part, the light is brighter than in any other portion: now I want you to tell me by what stars that brightest part passes," they never hesitated at once to give me the place of that strongest light, and to give it almost or quite exactly where I was in the habit of drawing it myself. So they did also with the boundaries of this luminous arch. Sometimes the light did not pass, by a regular dimming from this central line to the boundaries; but there was on each side a sudden offset or decided dimming about half way from it to the outer limits, whence it dimmed gradually again to the boundaries. I have repeated markings of this offset on my charts. It will be readily seen that I was not only, once every night, on the ecliptic; but was, six hours from that, at a distance of 23 or 24 degrees from that line: when this occurred towards the south, there was, on that side of the arch, beyond its usual boundaries, a faint and very dim light, as if I was then looking laterally at the nebulous ring. I did not ever notice this on the northern side. The light of this luminous arch was, at midnight, of a uniform character from one horizon to the other; a cold, white light, exactly like that of the Milky Way: and the arch had at that hour an uniform width the whole way across except at the horizons, where it widened out slightly; the nebulous substance at those lower extremes doubtless giving me reflected light, by which its outer edges of very diffuse matter were visible to the eye. This last sentence will convey to the reader's mind the idea that I did not consider the whole of this luminous arch as being reflected light: and I certainly, at an early stage of my observations on the Cordilleras, did begin to query whether this nebulous ring was not giving me light independently of the sun: that is—whether it was not self-luminous! My reasons for suspecting

this arose from the great difference in color between the light near the horizon adjoining the sun, whether in the evening or morning, and in the other portions of the arch: and also from the most decided manner in which the former, which was evidently reflected light, usually turned towards me, whatever my position might be, but especially when I was farthest from the ecliptic: while the other portions of the Luminous Arch, though siding over towards me at my farthest removes, were influenced in a far inferior degree by my change of place. The light at the horizon next the sun, except in the middle hours of the night, was always a warm yellow light, such as it ordinarily has in our own latitudes: while in all other parts of the arch it was a cold, white light, exactly like that of the Milky Way. Indeed not only was this arch in color like the galaxy, but its brightness was often so great that the notes to my observations in the deep hours of the night several times say that it looked much like another Milky Way stretched across the sky.

One evening, while I was standing on the hill Ychinbía near Quito, at the summit of which my observations were made, I watched the sun's last rays on the snow-covered top of the distant Cayambi, a mountain scarcely inferior in elevation or grandeur to Chimborazo itself. All around me had, for a long while, been in shadow: the sun had set to the lofty peaks of Pichincha opposite to me, but still lingered on Cayambi, lighting it up with a yellowish hue: suddenly, the yellow light ceased, and the snow of Cayambi took a white color that chilled me to look at. I was struck with the great resemblance of this yellowish hue to that of the Zodiacal Light as seen over the horizons just after nightfall or before daybreak: while the subsequent white color exactly resembled that of the other portions of the Luminous Arch; or, in the dark hours of the night, of the whole of it, stretched across the sky.

The question agitating my mind in all these observations was an interesting one: for, if this white light was not reflected light but was an independent light, then we have ascertained a most important quality of nebulous matter: it must be self-luminous.

I could not, at any time, discover an interruption or break in the light of this luminous arch, such as might be produced by the shadow of our globe. I tried, first, without having ascertained exactly where such a shadow ought to fall, so as to avoid any self-deception on the subject; but I could see no shadow: then I noted, carefully, on the charts where the sun's opposition would fall, but still there was no shadow to be made out. A German observer—I believe it is Theodore J. C. A. Brorsen,—who has seen this luminous arch repeatedly in Germany, says that the place opposite the sun was always brighter than any other part: I could never see any thing like that; the arch, to

my eye, having a pretty uniform character quite across, except the yellow part at the horizons as already noticed. This yellow light, in a good sky, just after nightfall or just before day-break, would extend as high as my zenith, where it would imperceptibly change into the cold white light. In the former cases, it had the distinctions which I have made in my printed volume, of a great effulgence at the centre and the lowest part, and of a diffuse light without this: I sometimes thought that these parts were brighter than I had ever seen them at the level of the sea: but at Panama, on my way home, I witnessed such a splendid exhibition of Zodiacal Light as rather to make me change my opinion. I ought to add that, on this homeward journey, I saw this Luminous Arch, while at the level of the sea, at Payta, at Panama, off Jamaica, and off the northeastern end of Cuba. My eye by that time had become so experienced as easily to make it out in each of those places; and when, at Payta and at Panama, I pointed out this arch to others, they had no difficulty in, at once, making it out, and in giving me the places among the stars, of the central line and of the boundary on each side. I have little doubt that a person experienced in tracing it on the sky will be able to make it out in our own latitudes—say as far north as New York, in the evening in March. Brorsen informs us in the *Astronomische Nachrichtung*, No. 998, that he has seen the Zodiacal Light quite across the sky repeatedly at Septenberg in Germany: Baron Humboldt saw also a portion, if not all, of this Luminous Arch in 1803 in lat. about 13° N. (see *Astron. Nachrichtung*, No. 989): and I must have seen it repeatedly in my cruise in the Mississippi—for which see Report of Japan Expedition, vol. iii, pp. 55, 65 and 85, in all of which instances I was at a great distance from the ecliptic.

I had also at Quito one good exhibition of Zodiacal Light from the moon, the moon then being $1^d\ 1^h$ past full: also, on one occasion, at 9 30, had simultaneously the Zodiacal Light in the west from the sun and from the moon in the east, just before the rising of the latter: this light over at the east and west horizons being connected above by the Luminous Arch. I hope soon to be able to publish all the observations, 123 in number, made in this visit to South America: and also those of Mr. Herrick at New Haven and Prof. Moesta at Santiago in Chile made at the same time. But few of Prof. Moesta's have yet been received; but, both these and Mr. Herrick's sustain the conclusion in my published book, that, when the spectator is north of the ecliptic, the main body of the Zodiacal Light is north of that line: and south, when he is on its southern side.

The plate No. 7 accompanying this article is made up from the result of all my observations on this occasion. The full line *cccc*, on the chart, will show the course of the centre or the

strongest light of the luminous arch. The lines *aaaa*, *aaaa*, show its boundaries: the line *bbb*, the boundary of the very dim light seen occasionally while I was at my farthest remove from the ecliptic on its southern side. The diagram A on the chart is intended to show the comparative intensity of the light across the Luminous Arch: this intensity being according to the distance of the line *ddd* from *ee*. At B is another diagram in which the intensity is shown when the offset noticed above was seen;—the depth of the line *fff* from *gg* showing the relative brightness of the several parts. As to the actual brightness of this Luminous Arch, the following notes from my journal of observations may be satisfactory to the reader.

Oct. 7th, 1856. $0^h\ 20^m$. "The Luminous Arch at m* has the brightness of the Milky Way at *n n*." [My zenith was 2° from the line *cc*.]

Oct. 29th, at 9 30. "I compared the brightness of the Luminous Arch with the Milky Way and thought that at *o* it was fully as bright as the latter at *ppp* (quite a bright part of the galaxy) perhaps even at *r*." [The ecliptic was vertical—the sky remarkably fine for observations.]

Dec. 2d. $11^h\ 30^m$. "Compared the light with that of the Milky Way, and thought that the part *s* of the Luminous Arch was equal in brightness to that of the Milky Way at *s'*." [The night remarkably fine: my zenith 24° S. of *cc*.]

Dec. 26th, $7^h\ 10^m$. "I thought the light of the Luminous Arch at *s* was equal to that of the Milky Way at *t t*, which is a tolerably bright part of the galaxy." [Sky fine for observations: my zenith 6° S. of *ccc*.]

Dec. 27th, $0^h\ 45^m$. "The brightness at *u u u* I thought equal to that of the Milky Way at *v v* on the chart: [my zenith was 28° S. of *ccc*.]

Feb. 3d, 1857, $4^h\ 45^m$. "I thought the Luminous Arch at *w w* equal to that of the Milky Way at *x*: not quite so bright as at *y*."

Feb. 4th, $3^h\ 20^m$. "I thought the light of the Luminous Arch from *w* to *z* was equal to that of the Milky Way at *y*. [My zenith 7° S. of *ccc*.]

Feb. 18th, $10^h\ 45^m$. "Thought the light at m^1 was equal to that of the Galaxy at n^1: at m^2 it was equal to that at n^2."

Feb. 24th, $9^h\ 20^m$. "The light along the central part of the Luminous Arch from m^3 to m^4 was equal to that of the Milky Way at n^3 n^4:" [my zenith 24° S. of *ccc*.]

The Luminous Arch usually remained visible in the zenith and the western part of the sky some time (occasionally 15 minutes) after dawn had decidedly come at the eastern horizon.

I had also observations of other very interesting phenomena in the heavens and the atmosphere: but, though they are *akin* to this topic, I do not consider it best to introduce them here.

The data which we now possess on the Zodiacal Light, seem to lead inevitably to the following conclusions.

§ 1. *That this light is from a great nebulous ring.* If there were no interruption from the moon or from clouds, this Luminous

* See the accompanying star-chart.

Arch was seen, all through the night, spanning the sky. To give only one example of this kind: on the night of the 26th–27th December, 1856, I had five observations; namely, at $7^h\ 10^m$, 9^h, midnight, at 3 and at $4^h\ 45^m$, mean time. In each of these, the arch was complete across the heavens; so that each observation overlapped the other considerably. The western horizon, when I began observing, was only 17° 15′ from the sun: and the eastern, at my last observation, was but 19°: so that, during this night, with the exception of 36° 15′ at the sun, the whole circle was complete—its entire completeness being prevented only by the sun's superior light. Doubtless the same thing would have occurred every night, but for the moon and clouds.

§ 2. *This is a geocentric ring.* The parallelism of the boundaries of this Luminous Arch at all hours of the night seems to be conclusive on this point. During the nine months of observations the outer boundaries kept their parallelism all through the night, as the eastern skies rose, and as one portion of the heavens after another came into view, or, in the west, was lost to sight; which could be the case only on the supposition of a geocentric ring. If we make it heliocentric, it must be within or without our earth. If within, then we should not be able to see this light at all at midnight: if without, and the observations were just after nightfall, or before dawn, then we should have the end next the sun at a distance of 170 or 190 millions of miles, while the opposite end would be comparatively quite near to us: in which case, by the laws of perspective, the portion towards the sun would be narrowed down almost or quite to the vanishing point, while the part opposite to it, or next to us, would be of a great width; all which is contrary to fact as witnessed in these equatorial observations, and is also the very opposite to fact as the Zodiacal Light is seen in latitudes distant from the equator. At New York the Zodiacal Light appears as a cone with its wide part resting on the horizon, and its apex towards the zenith: but if we suppose it to be from a heliocentric ring, its apex should be at the horizon and its broad part high up in the sky.

In the 3d volume of the Japan Expedition Report, I have noticed the laws of reflected light as shown by Bouguer: but I must be allowed to introduce them here again, as in my new observations I studied the aspect of the skies with a particular reference to this subject, and with, as I thought, decisive results. It is a well known fact in optics that the larger the angle between the lines of incidental light and of light reflected from any object, the larger is the quantity reflected: and Bouguer has given a table with the number of rays out of a thousand, reflected at various angles from 0 up to 179°. In the diagram on Plate No. 5, of a heliocentric ring, E being the earth, and AB

the horizon $1^h\ 20^m$ after sunset, the angle between the incident ray from the sun to B and the reflected ray BE will be 15°; between the incident ray to F, and EF will be 25°; at H the angle is 36°; and higher up, at I it is 45°; and at K it is still larger. Thus the angles between the incident and reflected rays go on increasing from the horizon up to K. The table of reflected rays which Bouguer has furnished us as the result of his experiments shows but little difference of the amount of light reflected, where the difference in angularity is so small, as in these cases; but supposing that there is no difference at all and that the light is equal from B to K, this would be in opposition to the facts of the case, which are uniformly that the Zodiacal Light is very strong, often quite effulgent at B, and goes, losing its intensity as it ascends towards the zenith of the spectator. The fact is most clearly obvious, that the Zodiacal Light requires, by the laws of reflection, an hypothesis which will give us the angle between the incident and reflected light, at B greater than at F; at F greater than at K, and so diminishing upward; which is done, as we shall presently see, by a geocentric ring: a heliocentric ring must always give us the very reverse of this. This latter and long admitted theory would give us only a Zodiacal Light wide at the zenith and tapering to a point at the horizon next the sun: and moreover brightest above, and losing its brightness as it approaches towards the same horizon: both of which are utterly contrary to facts.

In Plate 5, we see what the shape of this Luminous Arch in the figure AKIB, BDC would have to be, if the ring were heliocentric; AB being the horizon just after nightfall or before dawn. The part B of the Zodiacal Light towards the sun would be extremely narrow, almost or quite a vanishing point, while the part at A would have the width AC—which we may suppose to represent the breadth of the nebulous ring.

The following plate, No. 6, will help to show how these laws of reflection, as drawn out in Bouguer's tables, apply to a ring having the earth for its centre; and how completely, in this case, they coincide with the facts of the Zodiacal Light, as exhibited in any latitude. The reader, if he is not familiar with this phenomenon may test the subject by using Prof. Olmsted's plate of the Zodiacal Light in the volume of the Am. Journ. of Science for 1851, p. 309, which is a fair exhibition of the Zodiacal Light in high northern latitudes; at the equator the application of these laws of reflection is much more striking and decisive.

In this plate, the inner belt BE, HI, AK, may be considered as representing the Luminous Arch presented to the eye. If AB be the spectator's horizon an hour and twenty minutes after sunset—say at $7^h\ 20^m$, then CI*m*, BH*m* may represent the shape of the Zodiacal Light as usually shown in latitudes somewhat

removed from the equator. At Quito I had the portion CI as represented here, from causes hereafter explained; above I the Arch had a uniform width across to the horizon at A. As the night wore round and the western horizons changed to F, D, H, and finally to J, &c.—the opposite end of the Luminous Arch would change from A, to *b*, *c*, *d*, *e*, and finally to *f*: so that during the night the whole circle would be seen, except B*f*, which if the diagram here were entirely symmetrical* would be seen to be equal to 40°. The dotted lines S, S′, S″, &c., represent the sun's rays, and the angles S′BO, S″FO, S‴DO, &c., will be the angles between the incident and reflected rays, the spectator being at O. The following table will represent the value of these angles, and will show, according to Bouguer's tables, the number of rays reflected to the eye out of every thousand incident rays.

Angles—degrees.		Rays reflected from smooth water.	Rays reflected from plate-glass not quicksilvered.
S′BO,	161	343	422
S″FO,	146	184	270
S‴DO,	131	101	162
S⁗HO,	116	59	105
S⁗′ at midnight,	90	18	25
S⁗′*m*O,	67	18	25

The harmony between either of these columns of reflected rays, and the intensity of the Zodiacal Light from the horizon upward, is very remarkable. We do not know fully what nebulous matter is; but we may reasonably conclude that the laws for reflection of light, which Bouguer has discovered to apply to all other substances, apply also in this.

In my late observations, this Light was always brightest at the horizon and always brighter the nearer my horizon was to the sun: I studied it, night after night, with reference to these laws of reflected light, and constantly with an increasing conviction that a geocentric ring is the only one that could admit the phenomena presented to my eyes.

§ 3. *The position of this ring as regards the ecliptic.* In deciding upon this point, I shall be governed by the central line, or line of strongest light; and this was usually pretty well marked on the sky; so decidedly marked that the three gentlemen whom I have already spoken of as having been suddenly added as observers, had no hesitation in immediately stating its position among the stars, although they had never before noticed this Luminous Arch; and had now received no further aid from me, than to direct their attention to it, and to ask them to observe that the central part was the brightest portion of all. Since the whole of this arch followed slightly in my direction, as I was carried in a single night by the axial motion of the earth to the north or

* It is not so, the earth being disproportionately large.

south of the ecliptic, it is obvious that to get the position of this central line correctly, it ought to be observed only when it is nearly or quite vertical to the spectator. The average of all my observations, when I was at 2° or less from it, place it at its furthest extent on the south at 3° 10′ from the ecliptic. On the northern side I had but one observation of this kind, which made it 3° 30′ from the ecliptic. From this we may conclude that this central line; and therefore, as the outer boundaries always kept pace with it, that this great nebulous circle, makes an angle of 3° 20′ with the ecliptic.

My first observations of this central line placed the ascending node in longitude 62°; but subsequent ones made it vary from this down even to 42° of longitude: and the place of this crossing of the ecliptic seemed always to me to be very uncertain. But it was not so with the descending node, which every night's observation of that part of the sky showed at once to be near β Scorpii, in longitude 242°. All the three observers already adverted to, dictated this line to me as passing by the twin stars at that place, and I think we may accordingly consider the longitude of the nodes as at 62° and 242°.

§ 4. *The distance from the earth and the width of this ring.* The manner in which that portion of the Zodiacal Light which was obviously reflected light, was turned in my direction when I was at my farthest removes from the ecliptic, was, on most occasions, very remarkable. Sometimes it appeared to have little affinity with my change of position; but at others this warm, yellow light over the horizon was deflected towards me in a most decided manner, showing that the substance from which it was reflected could not be at any vast distance, most certainly not at the distance which a heliocentric ring would require it to be. For, my removes from the ecliptic could never be greater than 1421 miles: and a light affected so decidedly by so small a change in the observer's place could obviously not come reflected from a medium at twice the distance of the sun.

Some approximations towards the actual distance of the ring may perhaps be gained by noticing how the central line of strongest light was affected by my changes of place. This line always changed with me, going in the direction in which I was carried, which of course is the very opposite of parallax: and we must despair of having any parallax in Zodiacal Light. This central line varies very much on my charts, even when I was in the same positions; for it was extremely difficult to mark accurately an object oftentimes so uncertain; and my aim was, not uniformity, but to record facts as they appeared to my eyes. There are, in consequence, discrepancies among my lines. But still these facts admit of classifications, from which general deductions may be drawn. For example, my records from Feb.

15th, 1857 to May 23d, show me, by the diurnal revolution of the earth, in positions at the times of observation, from 23° 15′ N. to 19° 45′ south of the ecliptic, making a distance of 2580 miles. Taking all my observations during this period, north of 18° N. of the ecliptic, and noting the average of the positions of the central line for these observations: and then all south of 18° S. of the ecliptic, with also the average of the positions of the central line for these, I find that the central line is affected to the extent of 1° 45′ by these my changes of place: that is, supposing the average of the northern positions to have been 20° N. of the ecliptic, and those of the southern 20° S. of it, my change from one to the other, amounting to 2400 miles, produced a change in the central line of 1° 45′; not a parallax, but *towards* me as my place was changed. In my observation of the 7th March at 4 50 A. M. I was 19° 30′ N. of the ecliptic: in another at 9 P. M. of the 19th of that month I was 20° S. of that line, making a difference of 2370 miles between these two positions: the central line of the Luminous Arch shows a change in these two observations of $2\frac{1}{2}$ degrees; that is, as I went from north to south 2370 miles, the central line went with me southwardly $2\frac{1}{2}$ degrees. I should however prefer the former result of 1° 45′ for 2400 miles, the average of many results being more reliable than that of a few. From this, calculations may perhaps be made that will give us the proximate distance of this ring from the earth.

Another observation bearing on this was the one I had of a moon's Zodiacal Light on the 14th October, 1856—one day after the moon had become full. I have not the slightest doubt that it was Zodiacal Light produced by the moon. It was a strong, full light, with all the characteristics of this light when produced by the sun, except that this was white, and wanted the warm tinge of the Zodiacal Light from the sun. I took outlines of it, commencing at $6^h\,40^m$ mean time and ending at $6^h\,54\frac{3}{4}^m$; the moon being hid, for a while, by a cloud on the horizon. This light, pale and strong below, was merged above in that of the Luminous Arch. If the full moon gives us a Zodiacal Light, then the ring must obviously lie between that satellite and our earth.

§ 5. *The width of this ring.* My observations on this subject also show discrepancies which it is not easy to reconcile. During the first two months of my residence on the Cordilleras, the width of the Luminous Arch as recorded on my charts is about 20°; in November it increases to 30°; from Feb. 18 to May 23, the average of widths is 33° 58′; the average of the whole amount of my observations is 25° 56′ for the width. At any one night it had a uniform width quite across, except at the horizons. The increase of width in the whole number of observations is probably owing to the fact that my eye, becoming more experienced in

observations of the Luminous Arch, was able to trace out, better, its dimmer markings on the sky. We may perhaps estimate, with safety, the width of the Luminous Arch as decidedly given to the eye, 30°: but doubtless the nebulous matter of this ring extends beyond that; of which we have proofs in the much greater width of the Zodiacal Light near the horizons, where the sun, bringing the very diffuse matter at the edges into vision, shows us what the eye in other parts of the arch was not able to detect.

As respects the singular offset, or sudden change of brightness repeatedly noticed within the outer boundaries, as if there might be two rings, the one nearer to us and of greater brightness than the other, I will not venture an opinion, with the slight data now in my possession. The offset was not always perceived; but, when seen, was of so decided a character as scarcely to leave any doubt of its reality. This inner ring, as I will call it for sake of distinction, had an average width of 14° 54′.

The very dim light frequently seen beyond the usual boundary on the south when I was at my furthest removes from the ecliptic in that direction, seems also scarcely to admit of a solution, with our present knowledge of the subject. This dim, lateral light has, on my charts, a remarkable uniformity of breadth; never less than twenty-three or greater than twenty-seven degrees. The reader will remark that I saw it only on the southern side of the Luminous Arch.

§ 6. *The amount of matter* in this nebulous ring must be prodigiously great. Estimating its width at 30° and its distance from us at 100,000 miles, its width would be 52,600 miles: its thickness must, at present, be a subject only of conjecture: but at any thickness which dynamics will admit of, the amount of material there must be immensely great. It will be remembered that the width, 30°, is only that of the Luminous Arch as presented to the eye: beyond its borders, there seems still to extend nebulous matter too diffuse to be seen except by the power of the sun.

§ 7. The last sentence above would indicate that I consider *this nebulous matter as self-luminous, as well as capable of giving reflected light*:—to which opinion I am very strongly inclined. I had observations of other phenomena tending to confirm this opinion: but to notice them would lead me quite beyond my present limits, and into another subject.

§ 8. The opinion which I advanced at the meeting of the Scientific Association in Providence two years ago,—that this ring may be the source of our *aerolites*, seems to have much to support it: but there is not room to discuss it here.

Brooklyn, N. Y., Sept. 11th, 1857.

Art. XXVIII.—*On a New Source of Electrical Excitation;* by Mrs. Elisha Foote.

(Read before the American Association at Montreal, Aug. 13th, 1857.)

I have ascertained that the compression or the expansion of atmospheric air produces an electrical excitation. So far as I am aware this has not been before observed, and it seems to me to have an important bearing in the explanation of several atmospheric and electrical phenomena.

The apparatus used was an ordinary air-pump of rather feeble power and adapted either to compress or exhaust the air. Its receiver was a glass tube about twenty-two inches in height and three in diameter, with its ends closed by brass caps cemented to it. At the bottom was a stop-cock and a screw by which it was attached to the air pump. To the top were soldered two copper wires, one hanging down within the tube, terminating in one or more points and extending to within about six inches of the bottom, the other extending from the upper side of the cap to an ordinary electrical condenser.

In experimenting after compressing or exhausting the air within the receiver, the wire reaching to the condenser was disconnected from it. The upper plate was lifted from its place by its glass handle and its electrical condition tested by a gold leaf electrometer. I have found it convenient first to compress the air and close the stop-cock, when the condenser would be found to be charged with positive electricity. Then after discharging all traces of it both from the condenser and the wire leading to it, the air was allowed to escape, and the condenser would become recharged to an equal extent.

My experiments with this apparatus have extended over about eight months, and I have found the action to bear a strong analogy to that of the electrical machine. In damp or warm weather little or no effect would be produced, whilst at other times, particularly in clear cold weather, the action would be so strong as to diverge the leaves of the electrometer to their utmost extent. In warm weather when no action would be produced, I have attained the result by cooling the air artificially. A sudden expansion or contraction always increases the effect.

The results with oxygen gas were similar, but I was not successful with either hydrogen or carbonic acid gases.

It is believed that the results which have been obtained on a small scale in my experiments may be traced in the great operations of nature. The fluctuations of our atmosphere produce compressions and expansions sufficient to cause great electrical disturbances. Particularly should this be observed in the dry cold regions of our atmosphere above the effects of moisture and

vapors; and it was established by the experiments of Becquerel as well as those of Gay Lussac and Biot that the electricity of the atmosphere increases in strength with the altitude.

A manifest relation, moreover, between the electricity of the atmosphere and the oscillations of the barometer has frequently been observed. Humboldt, treating upon the subject in his Cosmos, remarks among other things that the electricity of the atmosphere, whether considered in the lower or the upper strata of the clouds in its silent problematical diurnal course, or in the explosion of the lightning and thunder of the tempest, appears to stand in a manifold relation to the pressure of the atmosphere and its disturbances.

The tidal movements of our atmosphere produce regular systematic compressions twice in twenty-four hours. These occur with so much regularity within the tropics, as observed by Humboldt, that the time of day is indicated within fifteen or twenty minutes by the state of the barometer. And Saussure observed a diurnal change in the electricity of the atmosphere corresponding with the diurnal changes of the barometer. The electricity of the atmosphere, he observes, has therefore a daily period like the sea, increasing and decreasing twice in twenty-four hours. It, generally speaking, reaches its maximum intensity a few hours after sunrise and sunset, and descends again to its minimum before the rising and setting of that luminary.

Other phenomena, which it is believed may be traced to the same cause, will be the subject of another communication.

ART. XXIX.—*On the Quantitative Assay of Chromium by Blowpipe Processes;* by EUGENE W. HILGARD, Ph.D.—(Abstract.)

(Communicated at the Montreal meeting of the American Association, Aug., 1857.)

THE quantitative determination of chromium has thus far been mostly confined to the laboratory. The processes employed were rather long, especially when many other elements occurred with the chromium, giving rise to a great variety of methods, requiring apparatus too voluminous to be carried in the field. No general method sufficiently simple and expeditious for practical purposes has been devised—methods such as we possess for most of the useful metals, and which the genius of Plattner has so admirably adapted to performance by the aid of the blowpipe. *Expeditiousness of execution* and *compendiousness of the utensils* required, are the characteristics of these processes of Plattner, and the main conditions of their practical usefulness; and it is to the exclusive employment of the dry way that these advantages are mainly owing. The metals being, after a

simple fusion, obtained in the shape of beads, does away with the tedious processes of filtration and evaporation; and if in a few cases the results thus obtained are not quite as accurate as those arrived at in the wet way, they are quite sufficiently so for all practical purposes. With metals difficultly fusible and reducible, which can not be brought into any available alloy (such as, besides others, iron and chromium), we are obliged partially at least to resort to the wet way. A mixed method of this kind has been proposed by Plattner for iron; and the one I now propose for chromium is of a similar kind. It is primarily intended for use in the field; also by the manufacturer to whom examinations of complex residues are frequently recurring; similarly it may serve to determine quickly and with considerable accuracy the value of the commercial preparations of chromium. In this I have not, however, invented any process essentially new. I have only attempted to adapt to the particular purpose in view, phenomena well known before, and there is undoubtedly room left for a good deal of improvement as it is. In describing the particular manipulations, I shall suppose the reader to be acquainted with the micro-laboratory of Plattner (his *blowpipe chest*), and his book on the same subject.*

The first step, as may be supposed, is the conversion of the chromium contained in the ore into chromic acid by fusion with alkalies and the simultaneous action of oxydizing agents, by which process chromium is at once separated from nearly all of the basic elements. In the qualitative test, this fusion is performed in the large platinum spoon, before the blowpipe flame. This process is always accompanied by mechanical loss from spirting of the flux, especially when *nitre* is used, without which, in most cases, the process is a slow and imperfect one. I found that a very slight admixture of phosphate of soda lessens the spirting very much; but I could find no means of preventing it altogether in an open vessel. I found it most convenient to use a platinum crucible of the same size and pattern as the clay ones ordinarily used; covered by a platinum torrefaction dish put on bottom downwards, and bent slightly out of shape, so as when set on to leave on one side a small orifice between itself and the rim of the crucible. The object of this orifice is to give exit to the gases, especially when, as sometimes happens, the flux inside rises so high as to reach the cover. But for this orifice (which must be kept very hot during the fusion) the cover would in such a case be lifted off by puffs, allowing the ejection of particles of the flux.

Of all fluxes hydrate of potash acts most powerfully on chromic iron ore; but in practice its use is inconvenient, partly

* The *spoon*, *lamp*, and other apparatus referred to beyond, are those of Plattner's *chest*.

on account of the excess of water the commercial reagent contains, and partly because of its powerful action on platinum, which would soon destroy any ordinary crucible. Carbonated alkalies are much more manageable; their effervescence with some substances is inconvenient at times; but there are cases in which they can not well be dispensed with. In the majority of cases, nitre alone answers perfectly, and it is decidedly the most convenient. The anhydrous potash formed in its decomposition does not act on the platinum perceptibly; it acts quite as powerfully as the carbonates; wherefore at present I generally confine myself to its use.

Supposing *chromic iron ore* to be under treatment; 100 milligrams of ore reduced to the finest possible division, are weighed off. The crucible is placed on the triangle of the lamp, and the ore being poured in first, three or four spoonfuls of nitre are added at once. The alcohol lamp is provided with its chimney, and so placed that by a simple movement of the support round its pivot, the orifice side of the crucible may be approached to or removed from the flame, which latter is so adjusted that the luminous cone is slightly truncated by the bottom of the crucible. The heat thus produced is quite sufficient in most cases; the rim of the crucible ought not be much heated, otherwise the flux will effloresce on the outside to such an extent as frequently to reach the triangle. If the latter is of iron, the experiment is lost; if of platinum, it may subsequently be washed off like the crucible itself. But with ordinary care, the accident is not likely to happen. In prolonged fusions, so much of the nitre is sometimes volatilized as to make it advisable to add some more. Care must be taken in lifting off the covering dish with the pincette, not to lose the drops of flux which usually adheres to its lower surface. If the ore has been sufficiently comminuted, its decomposition is usually complete in twenty-five to thirty minutes.

If the substance under treatment be *chrome ochre* or *chrome green* it is better to take only 50 to 75 milligrams for the assay; the nitre must not be added at once, but by half-spoonfuls, leaving the effervescence to subside each time before adding another, and allowing the fused mass to solidify on the surface. The heat must be very carefully regulated, taking as a guide the noise of decomposition. Unless this is attended to, the action is so violent as to render mechanical loss unavoidable. In these cases the completion of the oxydation is easily recognized; with the chromic iron ore, the only test is the absence of chrome in the washed residue. When the alkaline earths are present, either an alkaline carbonate must be used conjointly with the nitre, to insure the complete separation of the chromic acid; or else the substance may be treated like those containing lead, tin, bismuth, or cadmium.

Chromate of lead may be completely decomposed by fusion with nitre or the carbonates; but when the melted mass is treated with water, the reverse decomposition takes place, chromate of lead being again formed. Tin, bismuth, and probably cadmium behave similarly. This difficulty is best obviated by transforming those oxyds into silicates. The ore is first intimately mixed in the agate mortar with an excess (three to six heaped spoonfuls) of silica obtained in the wet way. A spoonful of nitre is then added and triturated with the mixture, whereby it is loosened from the mortar; it is then introduced into the crucible by single spoonfuls, as in the treatment of chrome green before described; after which the mortar is "rinsed" by triturating in it five or six successive half-spoonfuls of nitre. The flakes of silicate formed adhere to the sides of the crucible, appearing of a white or yellowish tint; if from incomplete mixture of the ingredients, any metallic chromate has remained undecomposed, it will appear of a dark red or orange on cooling. In this case no subsequent addition of silica will remedy the evil, and the experiment must be rejected. Most of these fusions are executed in from five to ten minutes.

The alkaline solution obtained by the treatment of the fused mass with water, may contain, besides the chromate, manganate, vanadate, tungstate, molybdate, titanate, silicate, antimoniate, arseniate, phosphate of potash, together with other non-metallic acids; those just mentioned being the most likely to interfere with the determination of the metal in view. It might be suggested to precipitate the chromic acid by Hg_2O, NO_5, when none of the above acids are present. Excellent as this method is when time can be allowed, its results fall seriously short of the truth when, as in the field, no delay is admissible. It would be better even to precipitate the chromic oxyd by ammonia, after reduction with alcohol. Neither of these methods is admissible when any of the acids mentioned are present.

It might be thought that a general method for the separation of the acids mentioned, would be found in the evaporation of the solution to dryness with HCl, and subsequent fusion with cyanid of potassium and an alkaline carbonate. But the oxyd of chromium thus obtained always contains potash, the amount of which is quite indefinite.

Passing over to the opposite of this plan, we find in the anhydrous potash chrome alum, $KO, SO_3 + Cr_2O_3, 3SO_3$, a compound insoluble in an *acid* solution in which most of the acids are soluble. It is well known that when a solution of chrome alum containing an excess of sulphuric acid is heated to 200° Centigrade, a light green powder of the above composition separates, which is insoluble and undecomposable by water or acids, and is but slowly resolved into its immediate constituents by boiling

potash-ley. It is however only an indefinite quotum of the chrome alum in solution which is thus precipitated. If the solution be still further heated, until oil of vitriol volatilizes, the whole of the chromium is precipitated in the form of a salt of the same composition as above, but which is slowly resolved into KO, SO_3 and $Cr_2O_3, 3SO_3$ (insoluble) by *hot* water alone, while it is not changed by cold water. When ignited, sulphuric acid, escapes and $KO, SO_3 + Cr_2O_3$ are left behind. It is upon the formation of this salt that I have based a general method of determination; finding that its formation is quite independent of the presence of any acid, and many basic substances, so long as an excess of SO_3 and potash are present at the same time.

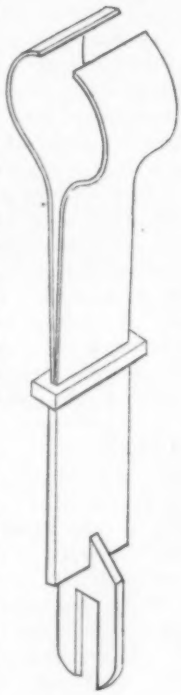
The solution containing the potash salts requires to be evaporated. This evaporation would be most safely and expeditiously performed in a long-necked glass flask, as with other voluminous filtrates; but it is difficult to find glass which will safely resist the subsequent bisulphate fusion. Even the porcelain dishes do not resist many of the latter. I have therefore contrived for the purpose a little evaporator of which fig. 1 is a half-size representation, and which is very useful in many other cases. The lower part, *a*, is of platinum, the upper, *b*, of thin glass; it is set on the lower vessel where the curvature of the rim begins. The seam between the two parts is effectually closed up by capillary attraction, as soon as any fluid is boiled in the vessel; so that the whole represents a close flask, which may be filled with fluid to within a few millimetres of the seam without any fear of loss, even if, in boiling, the froth should rise into the neck of the glass cover. Care must of course be taken not to heat the *rim* so as to cause boiling of the closing fluid.

1. (Half-size).

Silicic and manganic acids, when present in the solution, must be precipitated *before* filtering; the latter by a few drops of alcohol, and the former by a spoonful of nitrate of ammonia. In this case the solution must be boiled before filtration, to make the SiO_3 collect in flakes, otherwise there is difficulty in filtering. The crucible having cooled after the nitre fusion, is therefore transferred, together with its cover, to the evaporator, which is about one-third full of water. The solution may then be accelerated by heating, after which the precipitants just named are added and the whole boiled for half a minute, the vessel being covered with the watch-glass-shaped platinum cover used in the bisulphate fusion (see below); after this the fluid is transferred to one of the small beakers, vessel and cover being economically rinsed

and then set up for evaporation. Filtration and evaporation are to be carried on simultaneously. The platinum vessel is set on the grate of the lamp; the glass cover is clamped into the holder (fig. 2, full size), which in its turn is clamped into the same arm which carries the filtering support; the two parts are then joined and adjusted to the proper height.

2.



A two-inch filter is about the proper size in this case. Filters as small as these may be made so as to form an angle 5–8° larger than that of the funnel, so that while closely adjusted at the upper edge, their lower part hangs free within. The weight of fluid is insufficient to break down the paper, while filtration is essentially accelerated. So soon as the principal solution has passed through, it is poured into the evaporator from above (without soiling the neck) and evaporation begun. Previously, however, oil of vitriol is added until the solution becomes dark and free from chromic acid; then five to eight drops more, and finally three to eight spoonfuls of bisulphate of potash, the quantity varying *inversely* as the amount of nitre previously used. An excess does no harm. From time to time the filtrate is transferred to the evaporator, except the last one and a half or two fluiddrachms, which are reserved. The evaporation usually goes ahead of the filtration, and when finally the fluid in the evaporator is reduced so low as to evolve white fumes of nitric acid, the lamp is removed, the arm carrying the glass cover slightly raised, then moved sideways, and the glass unclamped. The latter is then rinsed into the platinum vessel with the reserved part of the filtrate, and finally and very economically, with the washing bottle. The whole of the rinsings ought not to fill more than one-third of the vessel, which is then covered with a watch-glass-shaped platinum cover, slightly smaller than the circumference of the rim, and not fitting too closely all around. Thus the fluid may be kept briskly boiling without loss; in a short time a frothing noise is heard, and shortly after fumes of oil of vitriol appear. At this period the clear green liquid suddenly changes color, and deposits a peach-blossom-colored powder, which on cooling turns light green. The heating is continued until *dense* fumes cease to issue; the fire being then removed, and the cover carefully laid aside, *upside*

down, the vessel is taken up with the pincers and the fused bisulphate made to spread itself on the sides as much as possible. Since no heat must be applied in the solution of the mass, it would be extremely tedious to wait for the solution of the dry bisulphate thus remaining. Five to six drops of oil of vitriol are therefore added, and the mass reheated with these; on cooling subsequently, it remains sufficiently soft to dissolve in water almost instantaneously. The cover is first rinsed into a small beaker, which may be filled to about one-third; the whole of this fluid is afterwards poured into the platinum vessel, and the solution promoted by stirring, so as to prevent any parts of the mass getting sensibly heated. When many foreign substances are present, it is best to add some more acid subsequently.

The precipitate cannot be washed with pure water. Salts, especially AmCl, prevent its diffusion in water, yet not sufficiently to make it wash expeditiously. It is necessary to involve it in some other precipitate settling quickly, not interfering with the separations, and volatile in the subsequent ignition. Such a one I found in the chlorosulphid of mercury. Half a spoonful of corrosive sublimate is added to the bisulphate solution, and while stirring violently, dilute sulphid of ammonium is added until, when the precipitate turns of a dark olive tint, the liquid above the flocculi is perceived to be clear. It may then at once be filtered; during the washing a little HgCl is thrown on the filter. Two and a half inch filters are best; the amount of ashes they give must of course be accurately known.

The filter is dried between blotting sheets, formed into a square package and incinerated in the platinum torrefaction dish, in a roasting furnace, after having driven off most of the mercury over the spirit lamp. The incineration is somewhat difficult; after it is complete, the loose mass is transferred to the platinum crucible previously counterpoised on the balance together with a small wire tripod (fig. 3). The crucible being covered with the torrefaction dish, is placed on the furnace, which is closed and provided with charcoal cover. A strong and steady heat is then maintained for four to six minutes, making the flame dart out at the chimney-hole. The mass will then appear dark green all over; if the heat has been strong, some chromate may have formed, which would vitiate the result. The whole is therefore moistened with a drop of strong sulphid of ammonium, then carefully dried over the lamp and gently re-ignited. Deducting the filter ashes from the weight of the residue, the result is calculated, 100 of residue corresponding to 47·3 of chromic oxyd.

3.

With proper care in the execution, the results obtained by this method are very satisfactory; generally rather a little too high, but rarely more than 0·1 milligram from the truth, and remarkably constant. The time required is generally about one and a half to two hours.

Of the acids above mentioned, I have not had opportunity of experimenting on the *titanic* and *vanadic;* but it is not likely that they would cause any difficulty in the separation. *Tungstic* acid remains with the green precipitate, and renders it flocculent, so as to make the use of the mercurial salt superfluous. After washing the filter a few times, six to eight drops of strong ammonia are poured on the filter, which is subsequently washed with an ammoniacal solution of sal-ammoniac. The separation is thus effected without difficulty. When *antimony* is present, the solution after the nitre fusion must be effected *without boiling*, and the residue washed with solution of nitre; if silica has to be precipitated, it must be made to collect in flakes by shaking in a test tube. After the bisulphate fusion, a spoonful or two of *tartaric acid* are thrown into the beaker before rinsing the cover. In the presence of copper, either silica must be added in the nitre fusion, or more simply, some caustic potash is added to the boiling solution, before precipitating the silica, if any. The chromate of copper then contracts into black scales of pure oxyd. The presence of a large amount of *magnesia* renders it advisable to add silica in the nitre fusion, lest too much of that earth should be carried over into the bisulphate solution by the ammoniacal salt. A small quantity does not vitiate the result. When the *halogens* are present, it is necessary to expel them before the nitre fusion. This is done by a preliminary fusion with a small quantity of bisulphate of potash, when chromic *oxyd* alone is present; if there is chromic *acid*, it must be previously reduced by heating with KCy or KO, OxO_3, otherwise the chromium will be partially volatilized. If simultaneously reducible metals are present which would endanger the platinum crucible, the fusion must be effected in a porcelain one. *Tantalic* and *niobic* acids would, like the tungstic, remain with the precipitate. As they are not likely to interfere often, I have not devised any method for their separation, which might require another filtration. The rest of the elements are separated without difficulty, in the ordinary course of the assay.

Art. XXX.—*Remarks on Fluorine: Action of Acids on Glass;* by Jerome Nicklès.

The usual process for detecting the fluorine in a compound not siliceous, is to set the fluorine free as fluohydric acid commonly from a vessel of lead or platinum and subject to its action a plate of glass. The process has been long in use, and is of value when there is much fluorine present. When the substance under examination contains only a trace of fluorine, and no corrosion of the glass is visible to the naked eye, it has been known that the effects of the reaction could be rendered visible by moistening the surface slightly with the vapor of water or with the breath; the parts acted on become distinct, and remain so as long as the slight film of vapor continues. This trial has till now been relied on with full confidence. On its indications, the presence of fluorine in many rocks and mineral substances has been admitted; and what is most to be wondered at is, that this element has not been found everywhere, since the reagents used in developing it themselves contain it.*

In fact, sulphuric acid, purified by whatever known process, may still contain some fluorine, which it has received through the nitrate of soda employed for oxydizing the sulphurous acid.

Again, another cause for the corrosion of the glass is found in the action of the acids themselves when in the state of a vapor, which, in cases like those under consideration, may sometimes equal that of fluohydric acid itself. The following simple experiment, which may be made in a quarter of an hour, proves this point. Cover a plate of glass, as usual, with wax,

* Since the publication of the above note in the Comptes Rendus for March, 1857, a reclamation of priority has appeared, which justifies what I have said. This reclamation is from Mr. Wilson, Professor of Chemistry at Edinburg, and it was presented to the Royal Society of that city. He there establishes that ten years since he found fluorine not only in the blood and all the liquids of the organism, but also in plants, various waters and minerals, that is, probably, wherever he sought for it. The same thing happened to me when I began my researches, and the difficulty was to find a substance that did not contain it. I found it in the crucibles which I used, and the capsules in which I evaporated the waters; but then I had not found it in the sulphuric acid used in my investigations. This I afterwards succeeded in doing, (see a paper beyond,) and this put a new phase on the problem, and I was led even to doubt its presence in bones. Before, I could obtain fluorine from the carapax of the lobster; but now, using sulphuric acid pure from fluorine, I find none in even 200 grams of the carapax. It is plain that if a fluoriferous acid be used, the presence of fluorine will be shown in the results. Moreover the vapors of sulphuric acid can act feebly like a feeble amount of fluohydric acid and produce an impression on glass appreciable after being breathed upon. As Prof. Wilson made his experiments with sulphuric acid, and with large quantities of it, he should have had traces of its presence. He states that he found fluorine in the waters of the Atlantic. I do not know on what scale he operated. But while at first I could find it in ten litres of the water, afterwards, on subjecting rock crystal to the vapors instead of glass, and using pure sulphuric acid, I found none with 300 litres.

and make a tracing of any regular figures: then fill a platinum crucible half full of sulphuric acid, and turn in some water sufficient to raise the temperature considerably, and cover the crucible with the glass, keeping the glass cool above by water. Add more water from time to time to the acid, as the temperature declines, until there is no further increase. At this point, the experiment is done; the lines of the tracing on the glass will be corroded distinctly to the naked eye. From such a result the presence of a notable proportion of fluorine might be inferred. But it is easy to prove the contrary: for if the acid be neutralized by an alkaline base so as to make a sulphate, and this salt be subjected to the action of an acid capable of displacing the fluohydric acid, the tracing is no more visible than it would be if the acid employed had acted itself alone, that is, it is visible only after moistening with the breath.

In the state of vapor, all acids will act with more or less energy on glass, and cause an etching,—a fact that should not surprise us when we consider how easily water will attack glass in powder. These effects take place, even when silica is present, which again proves that they may contain no fluorine. To obtain the best results, it is well to place the crucible in a sand-bath, and to keep the glass quite cool.

Two causes of error thus enter into the researches on fluorine, causes which the processes hitherto employed cannot avoid or set aside. The process I use is simple and serves to correct both assertions and errors. It consists simply in substituting rock crystal for the glass plate, the former resisting all action but that of fluohydric acid, and its sensitiveness to the latter being so great as to evince the presence of 0·000066 milligram of fluorine.

Among the errors to be corrected are those of my own, with reference to the presence of fluorine in bile, saliva, and gelatine. In ten grams of saliva I formerly obtained a decided tracing on glass. But with *pure* acid I now find none with sixty grams, or even ninety. Twenty litres of blood were necessary to obtain an appreciable effect from the fluorine.

But if there is fluorine in the blood, it should be found in alimentary substances. Still, I have not detected it in 200 litres of the waters of the Rhine, or of the Ill (Bas-Rhin), or in those of the Moselle, La Meurthe, La Meuse, La Seine; nor in 200 grams of the shells of oysters or of wood ashes: these substances still contain small quantities, and it is to be detected only by using much larger proportions. The most simple and economical means of discovering the presence of fluorine in waters is by the examination of the incrustations of boilers. These incrustations represent the product of the evaporation of a very large quantity of water, and they not only, in general, afford fluorine, but many other substances in small proportions.

With mineral waters it is often sufficient to employ a few litres only to find fluorine. Thus the mineral water of Plombières contains more than that of Vichy, and less than that of Selters or Antogast. It has been detected with four litres of the water of Plombières, eight of that of Vichy; while it is not distinguishable in eight litres of that of Niederbrunn, nor in sixteen of the water of Soultzmatt; and again it may be detected in one litre of the water of Châtenois. Thus while fluorine is generally present in mineral waters, the proportions vary largely. Besides, we may conclude that mineral waters rich in chlorids are not, as has been believed, a result of the infiltration of the waters of the sea.

ART. XXXI.—*On Fluoriferous Sulphuric Acid and its purification;* by JEROME NICKLÈS.

IT is now forty years since chemists were surprised by an announcement by Mullen, that when peroxyd of manganese or minium, free from chlorine, is treated with dilute sulphuric acid, a certain quantity of chlorine is evolved. It is useless to recall the hypotheses provoked by this curious reaction, appearing at a time when the nature of chlorine was little understood. At present, the mystery has passed away, and it is well known, that the chlorine was derived from a small quantity of hydrochloric acid existing as an impurity in the sulphuric acid of commerce.

But the fact respecting chlorohydric acid is now found to be true also of fluohydric acid; and it is evident from the preceding paper that already much confusion and error have arisen from this impurity in sulphuric acid derived from the nitrate of soda employed in its manufacture.

The practicability of purifying this acid I have proved on samples taken from various sources.

In my first trial I endeavored, in view of the volatility of this hydracid and its affinity for water, to drive it off by the heat of ebullition after diluting the acid with as much water. But it was a failure; the hydrofluoric acid was retained in consequence of the well known powerful action of mass, a large quantity of one substance (the sulphuric acid) controlling the other which was in so small proportions.

After various attempts, among which gelatinous silica was tried, I came to the following very simple process. In a porcelain capsule, or better still, in the bulb of a retort, the neck of which has been removed, a certain quantity of the acid to be purified was introduced, and diluted with twice its volume of distilled water; it was then placed in a sand-bath and heated

till the liquid had a slight movement, without actually boiling; this operation was continued for a greater or less time, usually thirty hours sufficing to obtain the sulphuric free from fluorine.

To test its purity the following method may be adopted. Put about thirty grams of the acid in a platinum crucible large enough to contain twice this quantity; add ten grams of distilled water, and immediately cover it with a plate of rock crystal which had been prepared with wax and traced over with regular geometrical figures,—such figures being more easily distinguished than others from any accidental striæ or markings. The rock-crystal should be kept cool with water, which for this purpose should be often renewed. The vapor of the water produced by the heat at the first contact of the water and acid, naturally condenses at first upon the cooled rock crystal. It is indispensable that this condition should be fulfilled; for the slight dew covering the bed of wax intercepts the fluohydric acid, which, as is well known, is very soluble in water, and acts only moderately on glass when it is free from humidity.

When the acid is so far diluted as not to heat up by the addition of a new quantity of water, it is necessary to have recourse to the lamp or sand-bath.

The temperature should be such that the hand cannot bear direct contact with the crucible. After two hours, the operation is completed. Then withdraw the crystal plate and remove the wax, wiping with care and cooling it. If there is no tracing apparent to the naked eye, and none while the film of vapor from the breath covers it, it gives evidence of purity from fluorine.

When, as in the old process, we operate with a plate of glass, and act upon carbonate of lime, such as calcined shells, shell of lobsters, &c., it takes but little time to obtain the corrosion; an effect which has, in fact, been already attributed to the presence of fluorine. This is what was done by M. Jenzsch, with the spars and aragonites,* and also by Mr. Wilson. These effects are easily explained; for a new affinity enters into play between the carbonates, an affinity which modifies that between the sulphuric and fluohydric acids, and the latter acid, as a consequence, cannot fail to be disengaged.

We may then, in the preceding experiment, use one of these agents, and the better to insure success, it should be moistened with a little distilled water. The experiment may then be finished in half an hour, especially if aided by heat. This process, based on the partial neutralization of the sulphuric acid under examination, is more sensitive and, especially, more expeditious than the preceding.

* Poggendorff's Annalen, 1855.

Art. XXXII.—*On Parthenogenesis.*

The subject of parthenogenesis in animals, and especially with reference to the Aphides or common plant-lice, was discussed in this Journal, early in 1854, by Dr. W. I. Burnett. Dr. Burnett brought to the question the results of his own minute researches, carried forward with the exactness and skill which are known to have characterized all his microscopic investigations. Since then, the subject has been growing in interest, and under its new name, parthenogenesis, it is taking more the aspect of a recognized principle in the Sciences of Life. The word, as its derivation implies, signifies the production of young by the female sex alone; and in the Aphides, generation follows generation, for a dozen or more, without renewed intercourse with the other sex.

Dr. Burnett appreciated the fact, that there were two modes of generation well known in plants and animals,—one by true eggs, the other by buds without eggs. The plant kingdom is known to be full of both processes. The bud from a branch, developing regularly its leaves, and capable as it often is of propagation when separated as well as when united to the original stem, is one variety of propagation by buds. The bud originating as a bulb at the axils of the leaves and branches, which drops off, and on finding soil, produces a new plant, is another variety of growth by buds. As each plant from such a bulb or bulbel will produce its crop of bulbels, propagation may be continued on, apparently indefinitely, without the necessary intervention of true flowers.

The Animal Kingdom in its inferior departments of the Radiates and Molluscs, exemplifies the same method of propagation. The young polyp may grow from the side of the old, and be persistent, like ordinary buds of plants, but become an independent individual if cut off; or in other cases, it may drop off on reaching towards maturity and thus acquire independence and so become the parent of a new zoophyte.

Thus far, the two kingdoms have been long known to be alike in reproduction. The bulbels of the plant are not like true seed in structure, neither are the bulbels of the Campanularidæ. There is not the germinal vesicle with its germinative spot. The development is simply gemmation. The analogies between plants and animals, it may be stated, go still farther: for as plants produce leaf buds, and then flower buds in which sexual organs and seed are developed, so some Medusæ (Tubularidæ) bud out polyps, to make the branching stems, and afterwards bud out Medusæ to develop sexual organs and ova: these Medusæ (as occasionally happens with the flowers of plants) separating and becoming free from the stalk that produced them. Moreover it

is now understood that the so-called alternation of generation is nothing more than the successive states exemplified in plants, of the embryo, incipient leaf bud, opened leaf bud, flower bud and flower, all of which are often widely diverse in forms.

It was in view of such facts as these that Dr. Burnett undertook to determine the nature of the process of continued non-sexual propagation in the Aphides; and his conclusion was, that the egg-like bodies, developed in clusters within the producing Aphis, were of the nature of buds, and not true ova, agreeing in this with Dr. Carpenter; and that the whole was analogous to the budding process. "The germs," he says, "have none of the structural characteristics of eggs, such as a vitellus, a germinative vesicle and dot; on the other hand, they are at first, simple collections, in oval masses, of nucleated cells." He also refers to the same kind of origin, the so-called hibernating eggs of *Daphnia* among Crustacea, *Lacinularia* among the Rotatoria, and *Hydatina* and *Notommata* among the Infusoria, in which, he observes, no germinative vesicle or dot has been seen, and no connection with the ovary discovered.

Parthenogenesis in the Aphides, according to this view, is reproduction by buds or gemmiparous reproduction, as distinct from sexual reproduction. It is like leaf-budding, the flower-budding (or sexual developments) taking place at longer intervals.

The later investigations of some zoologists have been tending to the conclusion that even true eggs, or bodies having the structure of eggs in every essential point, may be produced in some cases by females, and develop into perfect individuals without the intervention of the male, and without any proper hermaphroditism in the parent. The most important work that has appeared on this subject is one on "Parthenogenesis in Moths and Bees," by C. T. E. von Siebold of Munich, which has been translated in England by W. S. Dallas. The author describes the raising of brood after brood of young from some moths, without the recurrence of a single male; and in a *Psyche*, the pupa-case is filled with eggs before it is left; and in a *Selonobia*, the animal immediately after leaving the case, stuffs it full of eggs. The main point in his work, and one of more questionable character, relates to bees. He adopts the theory of a Silesian clergyman named Dzierzon, and after farther elaborating it, sustains the view that "the queen-bee, which like all other female insects, receives the seminal fluid of the male in a peculiar receptacle, there to be retained until it comes in contact with the egg during its passage through the oviduct, possesses the power of permitting or preventing this contact, so that the eggs may be deposited in the cells, either fecundated or unfecundated, at the pleasure of the mother; and farther, that from the fecundated eggs, female larves are produced which become developed either

into queens or workers, whilst the unfecundated eggs furnish the larves of the drones or males." The following are some of the points of evidence adduced in support of this remarkable theory, as given in a notice of the work in the Annals and Magazine of Natural History for July last.

"It is now generally admitted, even by bee-keepers, that the queen only copulates once, and that the supply of seminal fluid received at this time, and stored up in the seminal receptacle, serves for the fecundation of the immense number of eggs which she deposits during the period of her fertility, extending over several years. Sometimes, however, the stock of spermatozoids appears to be exhausted before the life of the queen comes to a close, and when this is the case she lays nothing but drone-eggs, introducing confusion into the wonderfully harmonious arrangements of the hive. This was found to be the case also with a queen which had been exposed to severe cold, with the view of destroying the vitality of the spermatozoids; of three queens thus treated, only one survived, and this afterwards laid nothing but drone-eggs. Another queen, whose abdomen had been injured so as *probably* to displace the seminal receptacle, also produced drone-eggs exclusively. Added to this, certain workers, which as is well known, are merely abortive females, destitute of copulative organs and of the seminal receptacle, and therefore incapable of fecundation, are found to possess imperfectly developed ovaries, which produce a very small number of eggs, and these, when deposited in the cells, are said always to produce drones. For most of these facts, von Siebold appears to have been indebted to the apiarians Dzierzon and von Berlepsch; but perhaps the most remarkable observations are those made by himself, in the microscopic examination of a considerable number of newly-deposited eggs. In the majority of the eggs deposited in worker-cells examined by him, he found spermatozoids; sometimes as many as four. In some instances these singular filaments still retained the power of motion. On examining twenty-seven drone-eggs laid by the same queen which had furnished a portion of the female eggs, von Siebold did not discover a single spermatozoid.

Such is the outline of the results at which the distinguished author has arrived; and although many will perhaps be disinclined to give an unhesitating adhesion to his views, there can be no doubt that his work is one of the most important that has appeared for a long time, one well worthy of being carefully studied by all physiologists, and one that must in the end greatly advance the cause of science, if only by calling the attention of observers to this singular and much neglected subject."

J. Lubbock, Esq., of England, has recently examined the so-called hibernating eggs of the Daphnia, and published his results in a valuable paper in the Philosophical Transactions, for 1857. These eggs, which he styles *agamic* eggs, are more abundant than the "ephippial" eggs, usually regarded as the eggs of the ordinary kind, and unlike the latter, they possess the power of development without impregnation. They are formed in the ovaries like true eggs, either in front of or behind the ephippial

mass; and moreover after close microscopic investigation Mr. Lubbock states that their formation appeared to commence from a cell or germinal vesicle like the ephippial egg, and differed little apparently in the process of enlargement excepting in the addition of one or more oil-globules. As the development proceeded, a very delicate membrane (vitelline membrane) forms about the egg. In about four days after a preceding brood had been produced, another brood was ready to enter the receptacle, the preceding having by this time developed sufficiently to swim about freely in the water. Mr. Lubbock remarks concerning the corresponding facts in the Aphides:

"If we except the non-essential difference, that in *Daphnia* the developing eggs do not alter their place in the ovary, the whole process as now detailed, is so similar to that which is described by Leydig in the Aphides, as to confirm in a remarkable manner his observations, and on the other hand, of course, to throw more doubt on the entirely different accounts of J. V. Carus and Dr. Burnett. I must, however, confess, that like the two last-mentioned observers, I have been quite unable to detect any germinal vesicle in the eggs of the viviparous Aphides; it is, however, very difficult to find even in those eggs which are destined to be impregnated in the ordinary manner, or indeed in any insect eggs, while, on the contrary, I have found it to be very large in all the eggs of Crustacea which have come under my notice."

Mr. Lubbock gives a brief notice of various papers bearing on the subject of Parthenogenesis, which we cite.

"The Crustacea are so seldom bred and watched in captivity, that although the Daphniadæ is the only family in the class which is as yet known to reproduce by parthenogenesis, yet the instances may hereafter prove to be much more frequent, as the fact that no males have yet been found of *Polyphemus oculus*, *Limnadia gigas* or *Apus* would seem to suggest.

"Similar phenomena have been recorded by Dumeril,* and more recently by Newman.†

"In the Insects, excepting the celebrated case of the Aphides and the genera *Cynips* and *Apophyllus*, as stated by Hartig,‡ whose assertions have been confirmed by the appearance of *Cynips lignicola* in great numbers in the southwest of England within the last year or two, where in several thousand specimens not a male occurred;—with these exceptions, I say, the phenomenon of parthenogenesis has as yet only been met with in the Lepidoptera, probably because this is the order most frequently bred by collectors.

"Pallas has observed this phenomenon in *Euprepia casta;* Bernoulli in *Gastrophaga potatoria* and *Episema cæruleocephala;* Basler in *G. quercifolia;* Suckow in *G. pini;* Treviranus in *Sphinx ligustri*, and

* Académie des Sciences, 28th Sept. 1856.

† An Essay on the employment of Physiological Characters in the Classification of Animals.

‡ Germar's Zeitschrift, vol. ii, p. 178.

Nordmann in *Smerinthus populi*. These instances are mentioned in Burmeister's Manual, p. 312; and Siebold* has attempted to explain them away, but in the present state of our knowledge the assertions are more probable than the explanation.

"Moreover, Siebold himself has observed parthenogenesis in *Solenobia lichenella* and *S. clathrella*,† in *Psyche helix* and in *Apis*.‡

"Lacordaire§ mentions the same fact as having occurred in *Liparis dispar* for three successive generations.

"Mr. J. P. Brown‖ has observed parthenogenesis in *Sphinx populi* and in *Arctia caja*, and states that M. Wagner has observed the same in *S. ocellata*.

"M. H. Lehocq¶ has met with a similar instance in *Arctia caja*; Dr. Kipp in *Sphinx populi*,** quoted by Siebold; Mr. Johnston in *Smerinthus ocellatus*;†† Mr. Curtis in *Bombyx polyphemus*;‡‡ and in *Bombyx mori* various naturalists have convinced themselves of the same facts.

"Mr. Westwood has favored me with the following extract from his unpublished notes:—'Mr. Davis (the publisher of the Entomological Magazine, and himself a good entomologist) informs me of a singular circumstance connected with the Egger moth, which has been observed by Mr. Tardy the Irish entomologist, after whom *Mesites Tardii* has been named, this gentleman having reared three generations of this moth from a single impregnation.'

"The Rev. P. H. Newnham of Guildford, the Rev. Hugh A. Stowell and Mr. E. W. Robinson have kindly informed me by letter that they have observed the same fact respectively in *Smerinthus populi*, *Arctia villica*, and *Arctia caja*.

"In *Psyche helix* and *Solenobia* parthenogenesis appears to be the rule, while in the other Lepidoptera it occurs as an exception; taking then these nineteen exceptional cases, it will be observed that they have all occurred in the hawkmoths or moths, and not one in the butterflies; four in *Smerinthus populi* and two in *S. ocellatus*, and three in *Arctia caja*; that, taking the genera, five are in *Smerinthus*, four in *Arctia* and *Gastrophaga*, and two in *Bombyx*. No doubt the silkworm moth (*B. mori*) has been so much and so carefully observed, that if parthenogenesis did occur even rarely in that species, it would certainly have been noticed by many observers; but I can suggest no explanation of the very unequal distribution of the other cases, except that agamogenesis does occur more frequently in some genera and species than in others, and *à fortiori*, that it takes place in some.

But if Professor Siebold and M. Dzierzon are correct, the Hive-Bee (*Apis mellifica*) presents us with the most wonderful instance of parthenogenesis as yet known among the Articulata, since in this instance the males always arise from unimpregnated eggs; and those eggs, which, if

* Wahre Parthenogenesis bei Schmetterlinge, &c.

† Bemerk. über Psychiden: Jahresbericht der Schlesischen Gesellschaft, &c., 1850; translated also in the Transactions of the Entomological Society of London, N. S. vol. vi.

‡ Wahre Parthenogenesis, &c.

‖ Mag. of Nat. Hist. viii, p. 557.

** Bienenzeitung, 1853, p. 1752.

‡‡ Ann. des Sc. Nat. 1851, p. 297.

§ Introd. to Ent., vol. ii, p. 383.

¶ Comptes Rendus, Dec. 8, 1856.

†† Zoologist, 1848, p. 2269.

impregnated, would have produced females, will, if no spermatozoa are present, give birth to males.*

"Several naturalists have convinced themselves that some plants have also the power of producing agamic seeds, and Gærtner has given an abridged account of the experiments on the subject.

"The plants in which the most complete experiments have been undertaken, and in which agamic seeds are supposed to have been obtained, are as follows:—

Zea mays, by R. J. Camerer, and again by Henschel; *Cannabis sativa*, by Spallanzani, Henschel, Giron de Buzareingues, Bernardi; *Spinacia oleracea*, by Spallanzani, Henschel, Giron de Buzareingues; *Cucurbita melopepo*, Spallanzani; *Cucurbita Citrullus*, Spallanzani; *Coyx Lacryma*, Henschel; *Lychnis dioica*, Henschel, Giron de Buzareingues; *Mercurialis annua*, Ramisch.

"Gærtner, however, is inclined to doubt the truth of the results obtained, and to ascribe the supposed agamic fertility either to pollen brought from a distance in the air, to the unnoticed presence of male flowers, or to 'crypto-hermaphroditism.' Mr. Smith, Curator of the Royal Kew Gardens, mentions a case of seeds which germinated without any apparent action of pollen as occurring in *Cælebogyne ilicifolia*, a native of Moreton Bay, and belonging to the Euphorbiaceæ, which was introduced into this country by Mr. Cunningham, who sent three plants to the Kew Gardens in 1829.

"These plants produced female flowers from which perfect seeds have been obtained for several successive years, although Mr. Smith could never detect 'anything like male flowers or pollen-bearing organs:' the ovarium and the seeds presented 'the usual structure of Euphorbiaceæ, such as it occurs in *Croton*, *Phyllanthus*, *Cluytia*, &c.'†

"Still more recently,‡ M. Henri Lehocq has observed the same phenomenon. He says, 'Mes expériences ont été faites sur le chauvre, l'épinard, le *Mercurialis annua*, le *Trinia vulgaris*, le *Lychnis sylvestris*, et sur une Cucurbitacée dont j'ignorais le nom spécifique. Je n'ai pas besoin de rappeler que j'avais pris toutes les précautions possibles pour isoler mes plantes, et cependant, à l'exception du Cucurbitacée et du *Lychnis*, toutes les autres me donnerent des graines fertiles.'§

"Dr. Carpenter,‖ and after him many authors, as for instance Dr. Burnett,¶ Carus,** and M. de Quatrefages, have sustained with ability the theory that the specimens which produce the so-called internal buds or gemmæ are not females, but entirely without sex. Thus, M. de Qautrefages†† says, 'Toutes les générations intermédiairies développées entre les termes extrêmes de ce cycle sont agames, c'est à dire, manquent de véritables organes reproducteurs, et se multiplient exclusivement par bouture et par bourgeon interne ou externe.'

"These and similar assertions, however true with reference to the Zoophytes, require great modifications with regard to the Articulata.

* Wahre Parthenogenesis, &c., *passim*.
† See Transactions of the Linnean Society, vol. xviii, p. 509.
‡ Comptes Rendus de l'Académie des Sciences, 8 Dec. 1856.
§ See also Journal of Microscopical Science, July, 1857, p. 228.
‖ *L. c.* p. 443. ¶ *L. c.* p. 96. ** *L. c.* †† *L. c.* p. 116.

"Prof. Siebold, who in his 'Bemerkungen über Psychiden,' in the 'Jahresbericht der Schlesischen Gesellschaft,' &c., 1850, p. 85, appears to have taken for granted that the self-fertile individuals of *Talæporia* (*Solenobia*) are, 'in ihrer organisation von weiblichen Thieren ganz verschiedene geschlechtslose Individuen' (in their organization from the female animals, entirely different sexless individuals), in his latest and very interesting essay, 'Wahre Parthenogenesis bei Schmetterlingen und Bienen,' admits that the self-fertile individuals in these insects are true females.

"This is also not the less evident in *Daphniæ;* for in them not only may the same specimen produce first agamic eggs, then ephippial, then again agamic, and then again ephippial eggs, so that it would have to be considered first sexless, then female, then sexless then female again, but actually, and that usually, the embryonic forms of both eggs are present at the same time."

"Throughout this paper I have applied the name of *eggs* to the ordinary reproductive bodies of the *Daphniæ*, although aware that it is customary to call all egg-like bodies which are fertile without immediate impregnation 'internal buds' or 'gemmæ,' and to consider them as essentially different from true eggs. Dr. Carpenter* and Dr. Burnett† have adopted this theory, and M. de Quatrefages also‡ says, 'Seulement chez les dernières [winged female Aphides, in opposition to the so-called sexless specimens] on trouve de véritables œufs, pourvus de toutes leurs parties caractéristiques; chez les premières de petites masses granuleuses, où l'on ne distingue jamais ni vitellus, ni vésicule germinative, ni tâche de Wagner.'

In this statement he probably relies on the descriptions of J. V. Carus and Dr. Burnett, but Leydig has already shown that there is good reason for supposing them to be in error; at any rate it is by no means applicable to the *Daphniæ*. In these we find in the ovary a number of ovarian masses containing small cells. One of these latter swells, dark granules collect around it, and it becomes a germinal vesicle. This is the foundation of an ephippial egg. Just in front, another cell swells a little, becomes surrounded in a similar manner by dark granules, and also by oil-globules, and this process continuing, forms the ordinary egg. I say *egg:* for how can we deny this term to a round body developed in the ovary of a female, and around a germinal vesicle?

"Truly has M. de Quatrefages observed,§ 'Ainsi l'on passe de la simple croissance d'un mammifère, au bourgeonnement le mieux caractérisé par des nuances insensibles: et tout nous ramène à cette importante conclusion, que le bourgeonnement, et par conséquent la réproduction agame, ne sont au fond qu'un phénomène d'accroissement.' But why stop the series here? Dr. Burnett says, indeed,‖ that 'the structure and conditions of all true ova are the same, and there is no passage between them and buds;' but this assertion is disproved by Leydig's observations on the Aphides,¶ and those now related in the *Daphniæ*. Prof. Owen, on the contrary, has maintained that buds and eggs are not bodies essentially different in their nature, but that we may expect to find every gradation

* See Dr. Carpenter "On the Microscope and its Revelations," p. 279.
† *Loc. cit.* ‡ *Loc. cit.* p. 121. § *Loc. cit.* p. 129.
‖ *Loc. cit.* p. 88. ¶ *Loc. cit.*

between them. It will probably ere long be convenient to apply some distinguishing name to those egg-like ovarium products which do not require impregnation as a necessary antecedent to development, but for the present, at any rate in the *Daphniæ*, we must call them eggs, which they more nearly resemble than ordinary gemmæ.

"Prof. Huxley might be supposed to agree with the naturalists as to the essential difference between eggs and buds above-mentioned, since he has proposed, developing the idea of which we owe the germ to Dr. Carpenter, to extend the use of the word 'individual' to the whole product of one impregnation, and to designate as 'zooids' the independent forms of the individual.* Prof. Huxley, however, proposes this system of nomenclature merely as convenient, and not as expressing any fundamental, structural, or potential difference between eggs and buds."

The investigations are still going on, and the subject is receiving thorough study. A paper on the topic was presented to the recent meeting of the American Association by Berthold Seemann which has not yet been published. We quote a single paragraph respecting Parthenogenesis in Plants from a paper by Dr. L. Radlkofer of Munich, published in the Annals and Magazine of Natural History, for September (xx, 204), relating particularly to the *Cœlebogyne ilicifolia*, which plant was first observed to be an example of parthenogenesis by J. Smith. (Linn. Trans., xviii, 509.)

"In the first rank here must be mentioned the observations on *Cœlebogyne ilicifolia*, a diœcious Euphorbiaceous plant, native of Australia, female specimens of which were long since introduced into England, and were widely distributed from there, before the male plant had been detected by travellers in its native country. No living specimens of the male plant have yet reached Europe; only a dried shoot with male flowers exists in the Herbarium at Kew. A glance at this suffices to show, from the composition of the inflorescence of the plants, the impossibility of the occurrence of a hermaphrodite flower in *Cœlebogyne*, and to show, further, that if the exceptional case which has been observed in other plants, of a production of male flowers on female specimens of diœcious plants, occurred in *Cœlebogyne* also, it could not be overlooked. Finally, all botanists who have had an opportunity of examining the female plants of *Cœlebogyne*, and among these are numerous authorities, agree in declaring that no male organs occur on them. In spite, however, of the fact that the exclusion of the fertilizing pollen of the same species must here certainly be most perfect, the plants cultivated at Kew annually ripen an abundance of seeds, from which even the third or fourth generation of (*female*) plants have been raised there."

The correctness of the conclusion was shown farther by an actual microscopic examination of the stigmas. The same paper gives details tending to sustain the conclusion that parthenogenesis occurs in the common Hemp (*Cannabis sativa*).

* Ann. and Mag. of Nat. Hist. 1852.

It would appear from observed facts, that among some of the lower animals, it is of no more account for one of them to bud out a complete animal of its kind than for a crab to reproduce its mutilated claw. Moreover, it seems to be also true that the budding process may take place in the ovary, and that it may evolve an egg or something very like an egg, thus commencing with the first step in the reproductive process; or it may evolve a bulb-like mass from other parts of the body, like that in ordinary gemmation; and each may develop into an individual animal, or what will produce such individuals. Whether formed in one place or another, a germinating cellule, or a spot or collection of cellules, begins the development, and the whole process from its initiatory step to the end is a regular growth from the single budding individual.

Moreover, the observations in the plant kingdom appear to show definitely, (confirmatory of Mr. Lubbock's observations in the Daphnia,) that in the case of ovary reproduction, the ovule which develops without impregnation is identical in its initial growth with that prepared for impregnation according to the ordinary seed-producing process. Yet, not to lose sight of the diverse relations of the two modes of reproduction, we should remember that, normally, in every species which buds or produces budding eggs, there are also the opposite sexes for true egg-development; that even the lowest sea-weed has its conjugation of oppositely related cells for spore reproduction; that reproduction of this one-sex kind is confined to the lower grades of species among animals, and some of these, like the Aphis, find the process so easy that they can turn off their germ-buds by the myriads, and still there is here a periodical recourse to the true sexual process; that in some animals like the Daphnia, while the ovaries produce eggs of both kinds, the normal eggs pass to another cavity and early show their distinctive character; that, in fine, a distinction of sex (a kind of sexual polarity) is the grand universal law for reproduction in life, and is never altogether set aside even for the inferior species, while absolutely essential in the higher. Moreover this supplemental and inferior means of propagation, or budding, is but an expansion of the ordinary law of growth: the same law that reproduces the nails and hair in man, the tail of a mutilated snake, or the legs of a maimed crustacean, evolves the polyp from the bud of a polyp, the Aphis from the Aphis germ-bud, or the plant from an unimpregnated ovule. The Aphis germ-bud or the unimpregnated ovule may be considered as only a minuter or more concentrated form of the bulb or bulbel.

The process by which the female produces the ovule which is afterwards to be impregnated is essentially a budding process, and not until after impregnation does it become in any case a

true developing egg; and it would seem that in a few cases, at least, it may develop either gemmately, or in true egg style, that is, it may continue a germ-bud or become an egg, according as it is or is not impregnated.

But while this extension of the budding method of propagation subserves an end of vast importance among the inferior animals and in the plant kingdom, it cannot be properly an equivalent to the normal sexual process. There is some great difference between what the female can bud out of herself, and what the sexes combined produce. It is probable from facts which have been observed both in plants and animals—though not yet demonstrated—that bud propagation will in all cases, if followed exclusively, end in the decline of the race, and its ultimate extinction; and that the sexes are required to keep up the sexual system and thus to sustain the type at its normal level and secure its perpetuity. This, if established as a real effect, is yet but a partial or inadequate expression of the difference between the two results. The subject opens a wide field for exploration.

One point seems clear, that the facts which are coming to light are calculated largely to extend and define former views, not to upset them. J. D. D.

Art. XXXIII.—*Correspondence of M. Jerome Nicklès, dated Paris, July 2, 1857.*

Obituary.—Three deaths of men of high importance in the scientific world, Cauchy, Thenard and Colla, have taken place since our last communication, two of them, in particular, an irreparable loss to science. We mention at this time some details respecting Cauchy, reserving for a future occasion our remarks on Thenard. M. Colla, director of the observatory at Parma, died rather suddenly at the age of fifty-one years, and though less eminent than the two just mentioned, his decease has occasioned no less regret.

Augustin Cauchy was born in August, 1789. He pursued his education for a while in the Polytechnic School, and at eighteen years of age left to enter the Department of Engineering. Soon after, he made his first publications, commencing with a chef-d'œuvre, a demonstration of Euler's celebrated theorem on Polyhedrons, supplying thus what had been wanting in geometry for 2000 years. Soon after, he gave a complete demonstration of one of the theorems left by Fermat to his successors, which the best geometers had hitherto demonstrated only as regards some particular cases. He brought out a beautiful theorem, on the number of values which a function can have when we permute its letters in all possible ways. This theorem enabled Abel, twenty years later, to prove that algebraic equations of the fifth and higher degrees could not be resolved.

Having been made a member of the Academy of Sciences in 1816, in place of the great Carnot, whom the government of the Bourbons had sent

into exile, Cauchy continued his publications with his former zeal. He issued five volumes of "Exercises Mathematiques," entirely his own, while at the same time he was performing the duties of Professor of Analysis and Mechanics in the Polytechnic School, supplying the chair of Poisson in Analytical Mechanics at the Faculty of Sciences, and also working upon his *Traité d'Analyse Algebrique*, and his *Leçons de Calcul differentiel et integral*, with three volumes of *Applications*. There are few questions in pure analysis or analysis applied to physics, mechanics or astronomy, on which he has not left traces of his rare penetration and superior talent.

A devoted partisan of the Bourbon dynasty, he followed it voluntarily into exile, and for some time occupied himself only with literature and poetry and the scientific education of the Count de Chambord. In 1837, he returned to France, when he resumed his academic chair, which, contrary to the rules, had been left vacant, protected from intrusion by the admiration which his genius had inspired.

With an active imagination, he rarely completed the works he began. He made a vow over the tomb of his father, that he would not permit himself to be led away by the fertility of his mind from finishing what he undertook: but notwithstanding his respect for the memory of his father, he did not keep his vow,—which is little surprising, considering that his temperament was so opposed to confinement for any length of time to one subject.

The name of Cauchy will remain associated with the greatest conquests in modern transcendental analysis. His determination of the number of real and imaginary roots of algebraic equations; his rigorous method of calculation by approximation for these same roots; his new theory of the symmetrical functions of the coefficients of equations of any degree; his mathematical theory of light, and particularly of dispersion, including his a-priori valuation without any previous photometric experiment and no data but two angles, sustain the exalted reputation with which he entered on his career.

But with a singularity not without parallel, he thought less of his mathematical genius and scientific works than of the scraps of poor verses which he composed at his leisure. He was not confident of being a good mathematician, while sure he was a great poet.

Augustin Cauchy died suddenly on the 23d of May, in consequence of a catarrhal affection which at first seemed of little moment, and which had not prevented his occupying himself to the last with a memoir that he read to the Academy on the 4th of the month. This memoir was on the use in astronomy of regulating coefficients, ("regulateurs coefficients"), an artifice in calculation on which he rested the highest hopes, classing it among the greatest discoveries.

Statues of Gay Lussac and Etienne Geoffroy St. Hilaire.—Gay Lussac died in 1851. Soon afterwards it was proposed to erect a monument to one who had rendered so eminent service to both theoretical and practical physics and also to chemistry. But his family, which was rich, objected to a subscription in its behalf; and they will erect one at their own expense. It will stand on the square between the Collège de France and the Sorbonne, in both of which institutions he was for a long time Professor.

Etienne Geoffroy St. Hilaire, who died in 1844, was not only the worthy rival of Cuvier, but he appreciated Cuvier before he did himself; he attracted him to Paris, and opened to him his career and education. His works are characterized by philosophical thought and a power of generalization which are becoming daily more appreciated. He was thus the means of creating two sciences, Philosophical Anatomy and Teratology. The monument to his memory will be erected at Etampes, where he was born in 1772. The expenses will be borne by a subscription.

History of Science. The Natural Method in Botany and Zoology.—All botanists are not of the opinion that the natural classification of plants by Jussieu is a work that will endure. It is true that the number thus thinking is a very small minority. Among them is M. Payer, a young botanist, who quite recently declared to the Academy of Sciences that the Jussieu system is an edifice tottering to its ruin. This opinion is not shared by his associates. The botanists present at the session, among them DeCandolle, protested against it, all agreeing that while Jussieu's system had undergone modifications made necessary by the progress of the science, it still stood, as chemical nomenclature stands to the honor of Lavoisier and Guyton Morveau. However an interesting fact was elicited by the discussion. It relates to the important part which Linnæus took in the establishment of the natural method in botany as well as in zoology. This was long since brought out by Geoffroy St. Hilaire.

Most authors date the natural system in zoology from a paper published by Cuvier and St. Hilaire in 1795. That system underwent three revisions by Cuvier, and in each he approximated to the Linnæan system; and finally he ended in reconstructing that system entire. G. St. Hilaire, perceiving the accordance, mentioned it to Cuvier; this was in 1827. The great zoologist was surprised, for he had thought that he had been following the ideas of Jussieu, and supposed he was going in an opposite direction from Linnæus. The following are the two classifications, as given by M. Geoffroy.

Classification of Linnæus.	Classification of Cuvier.
I. Primates.	II. Quadrumanes (Quadrumana).
II. Bruta.	V. Edentés (Edentata).
III. Feræ.	III. Carnassiers (Carnivora).
IV. Glires.	IV. Rongeurs (Rodentia).
V. Pecora.	VII. Ruminants (Ruminantia).
VI. Belluæ.	VI. Pachydermes (Pachydermata).
VII. Cetæ.	VIII. Cetacées (Cetacea).

The first order in Cuvier's system included the Bimana (Man).

Theory of Substitutions.—The theory of substitutions has been attributed by some to Dumas, and by others to Laurent, and others still to Gay Lussac. This question of priority, discussed by Laurent in his *Méthode de Chimie*, p. 241, has been recently taken up by Dumas, who has established the precise facts upon exact documentary evidence. It appears that the first idea of substitution is due to Dumas, who, on the 13th of January, 1834, made the formal statement that, "*when a hydrogenated substance is subjected to the action of any dehydrogenating substance, it takes up a portion of the latter, equivalent to that of the hydrogen lost.*" Dumas at this same time designated this class of phenomena

by the word *metalepsy*, an expression which has been attributed to Berzelius.

But the theory of substitutions required for its completion a correct knowledge of the part in the changes played by chlorine. It will be remembered that the idea of Laurent that "in the bodies obtained by substitution, chlorine not only takes the place of hydrogen, but acts the same part with it," met with general denunciation. It was followed by the bitter criticisms of Berzelius, Liebig, and Wöhler, and a declaration by Dumas disclaiming all participation in the view, which he called an "exagération outrée de sa théorie."

For some time Laurent was alone: but after a while his time of triumph came. The theory was then attributed wholly to him. But the note published by Dumas in the Annales de Chimie et de Physique, dispels all doubts and leads to justice being rendered to each of the two chemists,—to Dumas who opened the way, and to Laurent who established the theory and rendered it of practicable value.

Electric illumination.—We have spoken of the experiments in electric illumination made at Lyons by Lacassaigne and Thiers. They have continued their trials with great success, and through the month of March lighted one of the principal streets of Lyons, between the hours of seven to eleven. Two pieces of apparatus set up at the extremities of the street upon a frame crossing between the roofs of the opposite houses, sent their beams down the middle of the street. The gas was not lit; all the illumination was obtained from the battery. The Rue Imperial is about 550 yards long, and is lighted with more than forty jets of gas; and yet, the gas light was fully replaced by the electric light. Something still remains to be done. It is important that the light should come from a much greater height, that it may be more diffused, and less blinding and fatiguing to the sight.

Atmospheric electricity. Theory of thunder.—Among the theories of thunder, the recent one of M. Jobard, Director of the Museum of Industry of Brussels, should be counted; it may be called the chemical theory. According to it, thunder is nothing but the detonation of a detonating mixture of hydrogen gas, more or less carburetted, along with atmospheric air. The gas comes from decomposing organic matters and goes up in "pluies ascendants." Moreover he assumes that the same gas sustains the clouds, the water vesicles being filled with it. M. Jobard's fertile mind is so pressed with making theories for every thing that it does not give time for verifying them. He gets rid of the hydrogen again by supposing it to form water; and also *ammonia with the nitrogen* of the air.

New anesthetic agents.—Besides other agents proposed for replacing ether and chloroform in anesthesis, amylene ($C^{10}H^{10}$) has been proposed. It was discovered in 1844 by M. Balard, Professor in the Faculty of Sciences at Paris. It was first applied by Mr. John Snow, and for a while produced considerable sensation in the medical world. It may be questioned however whether it is better than aldehyde, artificial oil of naphtha, Dutch liquid, and other bodies of the class of ethers or hydrocarbons, which have been set down, on examination, as inferior to chloroform. However, according to the experiments at Paris of Dr. Geraldès, the amylenic anesthesis is more easily obtained than that by means of

chloroform; it is not preceded by convulsions, muscular contractions or stiffness of the limbs; the return to sensibility is prompt and complete; the action is quicker, but it is dissipated sooner, so that the breathing of the amylene must be carried on through the operation. Moreover, as amylene is very volatile (boiling at 31° C.), it is necessary in order to produce the respiration of it to return to the old methods, of late abandoned. If free from fatal mishaps in its use, there would be a disposition to favor it. But a disaster has just occurred in the hands of Mr. Snow, and this has deprived it of its highest recommendation, its innocuousness. It has been found that this amylene contains some amylic alcohol:—see beyond on the preparation of amylene.

Anesthesis by the Oxyd of Carbon.—This gas is used only externally, and on the diseased part. From the experiment by the unfortunate Chenot on himself, we know that internally it is a poison. According to the trials of M. Tourdis, of the Faculty of Medicine of Strasbourg, and afterwards of Dr. Ozanam, it has no effect when applied to the skin, unless the epidermis has been previously removed. But when applied to the skin from which the epidermis is removed or to a wound or sore, it acts efficaciously and produces perfectly the anesthetic effect; and in the clinics at Strasbourg, satisfactory results have been obtained. We add that ammonia appears to be an antidote to oxyd of carbon.

Anesthesis by Carbonic acid.—A physician at Paris, Dr. Follin, has recently endeavored to alleviate pain by means of a continued stream of carbonic acid gas. After the discovery of gases by chemistry, towards the close of the last century, medical men in England early undertook to examine the curative properties of different gases. The first experiments were made by Ingenhousz. He made known that a finger, when the epidermis was removed, had the pain increased by oxygen, but diminished and after a while removed by nitrogen, carbonic acid or hydrogen. This fact, according to this author, was then known in France, where it had been first observed.

It was soon after made useful in therapeutics; carbonic acid was recognized as the most effective gas for removing pain, and was employed with success in several cases of ulcers. But from 1794 to 1834, the facts seem to have been forgotten. At the latter date, M. Mojon, Professor at Genoa, proposed the use of this gas anew.

Dr. Follin was led to undertake his experiment from a knowledge of some recent trials by Dr. Simpson of Edinburg. He has applied a douche of the gas with success for the relief of pain, in the case of three females having an ulcerated cancer of the neck of the womb. Quiet followed the application in a few seconds and continued, in one case, for ten to twelve hours when a new application was required; in a second, eight days; and in the third, an intermediate interval between the other two. There was, however, no curative effect, which is not remarkable. Others have repeated the experiments. The gas must of course be made pure from any muriatic acid vapor, which may be effected by passing it through a concentrated solution of carbonate of soda. Dr. Follin prefers bicarbonate of soda for obtaining the carbonic acid, which he decomposes by means of tartaric acid.

Preparation of Amylene.—Dr. Duroy of Paris, who has been studying the best method of preparing amylene, states that there is great difficulty

in obtaining it pure. In the amylene of different shops, he has found the boiling point vary from 30° C. to 60° C., and has detected also the presence of more or less amylic alcohol which had escaped the action of the chlorid of zinc. The amylene received from Dr. Snow, varied in its point of ebullition from 30° to 46° C.; and besides, three grams treated with potassium disengaged sixty-four cubic centimeters of hydrogen, evidence that the liquid was oxygenated.

The author observed, at first, that on exposing amylic alcohol to dry chlorid of zinc, the alcohol gave the largest proportion of amylene, and that then it dissolves about one-tenth of its weight in chlorid. With the moist chlorid the amount of amylene is a little less, but the odor not so disagreeable. In either case, the author performs the distillation in a copper alembic, arranging the whole, as in Scottman's apparatus for ether. The product volatilized contains much amylic alcohol; it is rectified by several treatments with a dilute solution of chlorid of zinc, until potassium no longer disengages hydrogen, and then it is distilled from a water-bath, collecting only that which passes over at 30° C.

Miscellaneous Researches. (1.) *Siliciuret of hydrogen.*—Among the new facts of special interest in chemistry recently brought out, is the obtaining of the siliciuret of hydrogen, by MM. Wöhler and Buff, by passing an electric current through a solution of chlorid of sodium by means of electrodes made of siliciuretted aluminium. The gas inflames on contact with the air, and burns with a white flame, giving up silica. On passing it through an incandescent tube of glass, there is a deposit of amorphous silicium; and a deposit is also obtained when the flame is cut off by a porcelain capsule.

(2.) *Formation of Cane sugar from Glycerine, of Mannite, etc.*, by Berthelot.—After having succeeded in making glycerine and mannite to ferment, Berthelot thinking that before entering into fermentation, they probably pass to the state of *glucose*, a state which should be preceded by that of cane sugar, tried a great number of animal membranes and azotized ferments, and ended in finding in the testicular tissue a material fitted to produce rapidly this fermentation after it had been prepared by a previous contact with a fermenting substance.

(3.) *Researches on Tungsten.*—These researches were undertaken by M. Riche, Superintendent of the Chemical works of the Faculty of Science at Paris, to establish the position of tungsten in the series of simple bodies. According to him, it is a metalloid rather than a metal; and he concludes that although differing in some characters from boron and silicon, *it should be arranged along side of these metalloids.*

Bibliography. *Flore de Lorraine;* by A. Godron, Dean and Professor of Natural History of the Faculty of Sciences of Nancy. 2 vols. in 12mo, 2nd edition. Paris: chez J. B. Baillière.—M. Godron is one of the authors of the Flora of France, and the Flora of Lorraine has the characteristics distinguishing that great work. It contains also an exact account of the geological constitution of the regions growing the more prominent species of plants; and as the author believes that the chemical constitution of the soil exerts an important influence on growth, this point is also considered. The work was prepared especially for the students of the Faculty of Sciences of Nancy. It contains an analytical table as a guide to the families.

Etudes et Lectures sur les Sciences d'Observation et leurs applications pratiques; by M. BABINET. 3d vol. Paris: chez Mallet-Bachelier.—In this third volume, the author presents for popular reading, many subjects of importance, as follows. 1st. The Diamond and Precious Stones; 2d. Light-houses and artificial lights; 3d. Physics of the Globe (made up from the works of Mrs. Somerville); 4th. Quilliboeuf (consisting of results of meteorological researches by M. Babinet at the mouth of the Seine); 5th. The Mediterranean; 6th. On the Plurality of Worlds.

On the Growth of Wheat and on the Combined nitrogen in soils; by LAWES and GILBERT. 8vo, London. *On some Points in the Composition of Wheat grain*, its products in the Mill and Bread; by LAWES and GILBERT. 8vo.—Messrs. Lawes and Gilbert do not attribute to nitrogen in farina the importance acceded to it by chemists generally. They think that what is of special importance in aliments is a large proportion of the respiratory elements. The two pamphlets mentioned contain numerous analyses of the highest interest to agriculture and chemical science. Thus it is seen that both in Europe and America the proportion of gluten increases gradually in going from the north to the south; and that the quantity and quality of the ash which vary greatly when the crop is poor, seem to have in fully matured crops a uniformity of composition and of proportion which is very remarkable and almost independent of the nature of the manures used.

Maladie de la Pomme de Terre, de la Betterave, du Blé, et de la Vigne, de 1849 à 1853; par PAYEN. 1 vol., 12mo. Paris: chez Hachette.—Besides giving the history of the disease which has ravaged the crops of Europe, this small book points out the means for resisting it with success. Payen is member of the agricultural section of the Institute and General Secretary of the Central Society of Agriculture, and is in a better position than any one else to be well posted up in all that relates to agriculture.

La Baltique; par LEOUZON LE DUC. In 12mo. Paris: chez Hachette.—The author has for a long time lived in the Scandinavian countries. His work treats of the history of these countries from a political, literary, scientific and industrial point of view. The whole is clearly presented and in excellent taste. The work serves as a complement to the following—

Histoire des Etats Scandinaviens, Suède, Norwège et Danemark; par M. GEOFFROY, Professor in the Faculty of Letters at Bordeaux. 1 vol., contenant 5 cartes, 2 plans et une gravure.

L'Histoire d'Angleterre, comprenant celle de l'Ecosse, de l'Islande et des Possessions Anglaises; par FLEURY.—This subject is taken up from the same point of view as the above; and to the political and geographical divisions is added a literary and scientific, with a chapter on the part which the Anglo-saxon race has taken in the progress of human knowledge and in existing civilization.

La Musique mise a la portée de tout le Monde; par FETIS, Directeur du Conservatoire de Musique de Bruxelles. 1 vol., 2d ed. Paris: chez Hachette.—M. Fetis in his work presents with great simplicity the principles required to enable one to speak and judge of music without profound study. He commends it not only to the popular reader but to men of science, and especially to medical men who should understand the principles of acoustics.

Extracts from the Address of the President of the British Association, Rev. HUMPHREY LLOYD, *at the Meeting, Aug.* 26, 1857.

Astronomy.—The career of planetary discovery, which began in the first years of the present century, and was resumed in 1845, has since continued with unabated ardor; and since 1846 not a single year has passed without some one or more additions to the number of the planetoids; in one year alone (1852), no fewer than *eight* such bodies were discovered. The last year has furnished its quota of *five*, and in the present *three* more have been found, one by Mr. Pogson, of Oxford, and the other two by M. Goldschmidt, of Paris. The known number of these bodies is now forty-five. Their total mass, however, is very small. The diameter of the largest is less than forty miles, while that of the smallest (Atlanta) is little more than four.

These discoveries have been facilitated by star-maps and star-catalogues, the formation of which they have, on the other hand, stimulated. Two very extensive works of this kind are now in progress—the Star-Catalogue of M. Chacornac, made at the observatory of Marseilles, in course of publication by the French Government; and that of Mr. Cooper, made at his observatory at Markree, in Ireland, which is now being published by the help of the Parliamentary Grant of the Royal Society. It is a remarkable result of the latter labor, that no fewer than seventy-seven stars, previously catalogued, are now missing. This, no doubt, is to be ascribed in part to the errors of former observations; but it seems reasonable to suppose that, to some extent at least, it is the result of changes actually in progress in the Sidereal System. The sudden appearance of a new fixed star in the heavens, its subsequent change of lustre, and its final disappearance, are phenomena which have at all times attracted the attention of astronomers. About twenty such have been observed. Arago has given the history of the most remarkable, and discussed the various hypotheses which have been offered for their explanation. Of these, the most plausible is that which attributes the phenomenon to unequal brightness of the faces of the star which are presented successively to the earth by the star's rotation round its axis. On this hypothesis the appearance should be *periodic*. M. Goldschmidt has recently given support to this explanation, by rendering it probable that the new star of 1609 is the same whose appearance was recorded in the years 393, 798, and 1203. Its period in such case, is 405½ years.

The greater part of the celestial phenomena are comprised in the movements of the heavenly bodies and the configuration depending on them; and they are for the most part reducible to the same law of gravity which governs the planetary motions. But there are appearances which indicate the operation of other forces, and which, therefore, demand the attention of the physicist—although, from their nature, they must probably long remain subjects of speculation. Of these, the spiriform nebulæ, discovered by Lord Rosse, have been already referred to from this chair, as indicating changes in the more distant regions of the universe, to which there is nothing entirely analogous in our own system. These appearances are accounted for, by an able anonymous writer, by the action of gravitating forces combined with the effects of a resisting medium—the

resistance being supposed to bear a sensible proportion to the gravitating action.

The constitution of the central body of our own system presents a nearer and more interesting subject of speculation. Towards the close of the last century many hypotheses were advanced regarding the nature and constitution of the sun, all of which agreed in considering it an opaque body, surrounded at some distance by a luminous envelope. But the only certain fact which has been added to science in this department is the proof given by Arago that the light of the sun emanated (not from an incandescent solid, but) from a gaseous atmosphere, the light of incandescent solid bodies being *polarized by refraction*, while the light of the sun, and that emitted by gaseous bodies, is *unpolarized*. According to the observations of Schwabe, which have been continued without intermission for more than thirty years, the magnitude of the solar surface obscured by spots increases and decreases *periodically*, the length of the period being 11 years and and 40 days. This remarkable fact, and the relation which it appears to bear to certain phenomena of terrestrial magnetism, have attracted fresh interest to the study of the solar surface; and, upon the suggestion of Sir John Herschel, a photoheliographic apparatus has lately been established at Kew, for the purpose of depicting the actual macular state of the sun's surface from time to time. It is well known that Sir William Herschel accounted for the solar spots by currents of an elastic fluid ascending from the body of the sun, and penetrating the exterior luminous envelope. A somewhat different speculation of the same kind has been recently advanced by Mosotti, who has endeavored to connect the phenomena of the solar spots with those of the *red protuberances* which appear to issue from the body of the sun in a total eclipse, and which so much interested astronomers in the remarkable eclipse of 1842.

Next to the sun, our own satellite has always claimed the attention of astronomers, while the comparative smallness of its distance inspired the hope that some knowledge of its physical structure could be attained with the large instrumental means now available. Accordingly, at the Meeting of the Association held at Belfast in 1852, it was proposed that the Earl of Rosse, Dr. Robinson, and Prof. Phillips be requested to draw up a Report on the physical character of the moon's surface, as compared with that of the earth. That the attention of these eminent observers has been directed to the subject, may be inferred from the communication lately made by Prof. Phillipps to the Royal Society on the mountain Gassendi, and the surrounding region. But I am not aware that the subject is yet ripe for a Report.

I need not remind you that the moon possesses neither sea nor atmosphere of appreciable extent. Still, as a negative, in such case, is relative only to the capabilities of the instruments employed, the search for the indications of a lunar atmosphere has been renewed with every fresh augmentation of telescopic power. Of such indications, the most delicate, perhaps, are those afforded by the occultation of a planet by the moon, The occultation of Jupiter, which took place on the 2nd of January last, was observed with this reference, and is said to have exhibited no *hesitation*, or change of form or brightness, such as would be produced by the

refraction or absorption of an atmosphere. As respects the sea, the mode of examination long since suggested by Sir David Brewster is probably the most effective. If water existed on the moon's surface, the sun's light reflected from it should be completely polarized at a certain elongation of the moon from the sun. No traces of such light have been observed; but I am not aware that the observations have been repeated recently with any of the larger telescopes.

It is now well understood that the path of astronomical discovery is obstructed much more by the earth's atmosphere than by the limitation of telescopic powers. Impressed with this conviction, the Association has, for some time past, urged upon Her Majesty's Government the scientific importance of establishing a large reflector at some elevated station in the southern hemisphere. In the mean time, and to gain, as it were, a sample of the results which might be expected from a more systematic search, Prof. Piazzi Smyth undertook, last summer, the task of transporting a large collection of instruments—meteorological and magnetical, as well as astronomical—to a high point on the Peak of Teneriffe. His stations were two in number, at the altitudes above the sea of 8,840 and 10,700 feet respectively; and the astronomical advantages gained may be inferred from the fact, that the heat radiated from the moon, which has been so often sought for in vain in a lower region, was distinctly perceptible, even at the lower of the two stations.

Figure of the Earth and the Tides.—The researches relative to the *Figure of the Earth* and the *Tides* are intimately connected with Astronomy, and next claim our attention. The results of the Ordnance Survey of Britain, so far as they relate to the earth's figure and mean density, have been lately laid before the Royal Society by Col. James, the Superintendent of the Survey. The ellipticity deduced is $\frac{1}{289 \cdot 33}$. The mean specific gravity of the earth, as obtained from the attraction of Arthur's Seat, near Edinburgh, is 5·316; a result which accords satisfactorily with the mean of the results obtained by the torsion balance. Of the accuracy of this important work, it is sufficient to observe, that when the length of each of the measured bases (in Salisbury Plain and on the shores of Lough Foyle) was computed from the other, through the whole series of intermediate triangles, the difference from the measured length was only five inches in a length of from five to seven miles. Our knowledge of the laws of the *Tides* has received an important accession in the results of the tidal observations made around the Irish coasts in 1851, under the direction of the Royal Irish Academy. The discussion of these observations was undertaken by Prof. Haughton, and that portion of it which relates to the diurnal tides has been already completed and published. The most important result of this discussion, is the separation of the effects of the sun and moon in the diurnal tide—a problem which was proposed by the Academy as one of the objects to be attained by the contemplated observations, and which has been now for the first time accomplished.

From the comparison of these effects, Prof. Haughton has drawn some remarkable conclusions relative to the mean depth of the sea in the Atlantic. In the dynamical theory of the tides, the ratio of the solar to the lunar effect depends not only on the masses, distances, and periodic times, of the two luminaries, but also on the depth of the sea; and this,

accordingly, may be computed when the other quantities are known. In this manner, Prof. Haughton has deduced from the solar and lunar co-efficients of the diurnal tide a mean depth of 5·12 miles—a result which accords in a remarkable manner with that inferred from the ratio of the semi-diurnal co-efficients, as obtained by Laplace from the Brest observations. The subject, however, is far from being exhausted. The depth of the sea, deduced from the solar and lunar *tidal intervals*, and from *the age* of the lunar diurnal tide, is somewhat more than double of the foregoing; and the consistency of the individual results is such as to indicate that their wide difference from the former is not attributable to errors of observation. Prof. Haughton throws out the conjecture that the depth, deduced from the tidal *intervals* and *ages*, corresponds to a different part of the ocean from that inferred from the *heights*.

Terrestrial Magnetism.—The phenomena of *terrestrial magnetism* present many close analogies with those of the tides; and their study has been, in a peculiar manner, connected with the labors of this Association. To this body, and by the hands of its present General Secretary, were presented those reports on the distribution of the terrestrial magnetic force which re-awakened the attention of the scientific world to the subject. It was in the Committee-rooms of this Association that the first step was taken towards that great magnetic organization which has borne so much fruit; it was here that the philosophical sagacity of Herschel guided its earlier career; and it was here again that the cultivators of the science assembled, from every part of Europe, to deliberate about its future progress. It was natural, therefore, that the results obtained from such beginnings should form a prominent topic in the addresses which have been annually delivered from this chair; and the same circumstances will plead my excuse, if I now revert to some of them which have been already touched upon by my predecessors.

It has been long known that the elements of the earth's magnetic force were subject to certain regular and recurring changes, whose periods were, respectively, a *day* and a *year*, and which, therefore, were referred to the sun as their source. To these periodical changes Dr. Lamont, of Munich, added another *ten years*, the diurnal range of the magnetic declination having been found to pass from a maximum to a minimum, and back again, in about that time. But besides these slow and regular changes, there are others of a different class, which recur at *irregular* intervals, and which are characterized by a large deviation of the magnetic elements from their normal state, and generally also by rapid fluctuation and change. These phenomena, called by Humboldt "magnetic storms," have been observed to occur *simultaneously* in the most distant parts of the earth, and thus to indicate a cause operating upon the entire globe. But, casual as they seem, these effects are found to be subject to laws of their own. Prof. Kreil was the first to discover that, at a given place, they recurred more frequently at certain hours of the day than at others; and that, consequently, in their mean effects, they were subject to periodical laws, depending upon the *hour* at each station. The laws of this periodicity have been ably worked out by General Sabine in his discussion of the results of the British Colonial Observatories; and he has added the important facts, that the same phenomena observe also the two

other periods already noticed,—namely, the *annual* and the *decennial* periods. He has further arrived at the very remarkable result, that the decennial magnetic period coincides, both in its duration and in its epochs of maxima and minima, with the decennial period observed by Schwabe in the solar spots; from which it is to be inferred that the sun exercises a magnetic influence upon the earth dependent on the condition of the luminous envelope.

We are thus in the presence of two facts, which appear at first sight opposed—namely, the absolute simultaneity of magnetic disturbances at all parts of the earth, and their predominance at certain local hours at each place. General Sabine accounts for this apparent discrepancy by the circumstance, that the hours of maximum disturbance are different for the different elements; so that there may be an abnormal condition of the magnetic force, operating at the same instant over the whole globe, but manifesting itself at one place chiefly in one element, and at another place in another. I would venture to suggest, as a subject of inquiry, whether the phenomena which have been hitherto grouped together as "occasional" effects may not possibly include two distinct classes of changes, obeying separate laws: one of them being strictly periodic, and constituting a part of the regular diurnal change; while the other is strictly abnormal, and simultaneous at all parts of the globe. If this be so, it would follow that we are not justified in separating the larger changes from the rest, merely on the ground of their magnitude, and that a different analysis of the phenomenon is required. The effects hitherto considered are all referable to the sun as their cause. Prof. Kreil discovered, however, that another body of our system—namely, our own satellite—exerted an effect upon the magnetic needle, and that the magnetic declination underwent a small and very regular variation, whose amount was dependent on the lunar hour-angle, and whose period was therefore a lunar day. This singular result was subsequently confirmed by Mr. Broun in the discussion of the Makerstoun Observations; and its laws have since been fully traced, for all the magnetic elements, by General Sabine, in the discussion of the results obtained at the Colonial Magnetic Observatories.

The foregoing facts bear closely upon the debated question of the causes of the magnetic variations. It has been usual to ascribe the periodical changes of the earth's magnetic force to the thermic action of the sun, operating either *directly* upon the magnetism of the earth, or affecting it *indirectly* by the induction of the thermo-electric currents. Here, however, we have a distinct case of magnetic action, unaccompanied by heat; and the question is naturally suggested, whether the larger diurnal change may not also be independent of temperature. The most important fact, in its bearing upon this question, is the existence of an *annual inequality* in the diurnal variation, dependent on the sun's declination, which has been recently pointed out by General Sabine. If we deduct the ordinate of the curve which represents the mean diurnal variation for the entire year, from those of the summer and winter half-yearly curves respectively, the differences are found to be equal and opposite; and the curves themselves, consequently, are *similar*, but *oppositely placed*, with respect to the axis of abscissæ. From this, General Sabine draws

the inference, that the diurnal variation is a direct effect of solar action, and not a result of its thermic agency.

Light.—The most important of the recent additions to the theory of *Light* have been those made by M. Jamin. It has been long known that metals differed from transparent bodies, in their action on light, in this, that plane-polarized light reflected from their surfaces became *elliptically polarized;* and the phenomenon is explained on the principles of the wave-theory, by the assumption that the vibration of the ether undergoes a *change of phase* at the instant of reflexion, the amount of which is dependent on its direction and on the angle of incidence. This supposed distinction, however, was soon found not to be absolute. Mr. Airy showed that *diamond* reflected light in a manner similar to metals; and Mr. Dale and Prof. Powell extended the property to all bodies having a high refractive power. But it was not until lately, that M. Jamin proved that there is *no distinction* in this respect between transparent and metallic bodies; that all bodies transform plane-polarized into elliptically-polarized light, and impress a change of phase at the moment of reflexion. Prof. Haughton has followed up the researches of M. Jamin, and established the existence of *circularly-polarized* light by reflexion from transparent surfaces.

The theoretical investigations connected with this subject afford a remarkable illustration of one of those impediments to the progress of Natural Philosophy which Bacon has put in the foremost place among his examples of the *Idola*—I mean the tendency of the human mind to suppose a greater simplicity and uniformity in nature than exists there. The phenomena of polarization compel us to admit that the sensible luminous vibrations are *transversal*, or in the plane of the wave itself; and it was naturally supposed by Fresnel, and after him by M'Cullagh and Neumann, either that no *normal* vibrations were propagated, or that, if they were, they had no relation to the phenomena of light. We now learn that it is by them that the *phase* is modified in the act of reflexion; and that, consequently, no dynamical theory which neglects them, or sets them aside, can be complete.

Attention has been lately recalled to a fundamental position of the wave-theory of light, respecting which opposite assumptions have been made. The vibrations of a polarized ray are all parallel to a fixed direction in the plane of the wave; but that direction may be either *parallel* or *perpendicular* to the plane of polarization. In the original theory of Fresnel, the latter was assumed to be the fact; and in this assumption Fresnel has been followed by Cauchy. In the modified theories of M'Cullagh and Neumann, on the other hand, the vibrations are supposed to be parallel to the plane of polarization. This opposition of the two theories was compensated, as respects the results, by other differences in their hypothetical principles; and both of them led to conclusions which observation has verified. There seemed, therefore, to be no means left to the theorist to decide between these conflicting hypotheses, until Prof. Stokes recently, in applying the dynamical theory of light to other classes of phenomena, found one in which the effects should differ on the two assumptions. When light is transmitted through a fine grating, it is turned aside, or *diffracted*, according to laws which the wave-theory has

explained. Now, Prof. Stokes has shown that, when the incident light is *polarized*, the *plane of vibration* of the diffracted ray must differ from that of the incident, the two planes being connected by a very simple relation. It only remained, therefore, for observation to determine whether the *planes of polarization* of the incident and refracted rays were similarly related or not. The experiment was undertaken by Prof. Stokes himself, and he has inferred from it that the original hypothesis of Fresnel is the true one. But, as an opposite result has been obtained by M. Holtzmann, on repeating the experiment, the question must be regarded as still undetermined.

The difference in the experimental results is ascribed by Prof. Stokes to the difference in the nature of the gratings employed by himself and by the German experimentalist, the substance of the diffracting body being supposed to exert an effect upon the polarization of the light, which is diffracted by it under a great obliquity. I learn from Prof. Stokes that he proposes to resume the experimental inquiry, and to test this supposition by employing gratings of various substances. If the conjecture should prove to be well founded, it will greatly complicate the dynamical theory of light. In the mean time the hypothesis is one of importance in itself, and deserves to be verified or disproved by independent means. I would venture to suggest that it may be effectively tested by means of the beautiful *Interference-refractor* of M. Jamin, which the inventor has already applied to study the effects upon light produced by grazing a plate of any soluble substance inclosed in a fluid. It is well known that the refractive index of bodies increases with their density; and the theory of emission has even expressed the law of their mutual dependence. That theory, it is true, is now completely overthrown by the decisive *experimentum crucis* of MM. Fizeau and Foucault. It was, therefore, probable, *à priori*, that this law—the only one peculiar to the theory—should be found wanting. Its truth has recently been put to an experimental test by M. Jamin. Water, it is known, has its maximum of density at about 40° of Fahrenheit; and accordingly, if Newton's law were true, its refractive index should also have a maximum value at the same temperature. This has been disproved by M. Jamin, by observing the interference of two rays, one of which has passed through air, and the other through water; and thus the last conclusion of the emission-theory has been set aside.

It would occupy too much of your time were I to touch, even lightly, upon the subject of the chemical action of light, and the many beautiful and important discoveries of the art to which it has given rise. I may, however, mention, as one of the latest of the marvels of *photography*, that M. Poitevin has succeeded in producing plates in relief, for the purposes of engraving, by the action of light alone. The process depends upon the change in the affinity for water, produced by the action of light upon a thin plate of gelatine, which is impregnated with bichromate of potash.

SCIENTIFIC INTELLIGENCE.

I. CHEMISTRY AND PHYSICS.

1. *On the calculation of the densities of vapors.*—KOPP has suggested a mode of calculating the densities of vapors when referred to air as unity, which is shorter and more convenient than the ordinary process. If the equivalent of a body be referred to oxygen taken as 8, and its vapor-density to air as 1, we find that the quotient of the atomic weight by the density is equal to one of the following numbers,

28·88 14·44 7·22

The number 28·88 corresponding to a condensation of four, 14·44 to one of two, and 7·22 to one of one volume. The author calls these numbers normal quotients. When the density of the vapor of a substance has been determined approximately by experiment, the quotient of its equivalent by this density is a number which is very near one of the preceding. This operation shows at once the mode of condensation of the vapor. Conversely, it is always easy to calculate the vapor density by dividing the equivalent by one or the other of the normal quotients. Thus, for a great majority, if not for all organic compounds, the divisor is 28·88, because the substances correspond to 4 vols. Thus in the case of acetic ether we have for the equivalent 88 and for the experimental density 3·112, now $\frac{88}{3\cdot112}=28\cdot2$, a result which indicates a condensation into 4 vols. The theoretic density is therefore $\frac{88}{28\cdot88}=3\cdot047$.

This mode of calculating the vapor-density of a body does not require a knowledge of the densities of the vapors of the elements contained in the body. It is only necessary to know the equivalent of the body itself. It follows that bodies which have the same equivalent will usually have the same density of vapor. Thus phenol, $C_{12}H_6O_2$, and bisulphid of methyl, $C_4H_6S_4$, have the same equivalent, 94, and the same density, 3·255. When the densities of bodies having the same equivalent are not the same they will of course be connected by simple ratios.—*Comptes Rendus*, xliv, 1347.

[*Note.*—If we take the density as well as the equivalent and combining volume of hydrogen =1, the vapor-densities of all other gases and vapors are either equal to their equivalents or to their equivalents divided by a low number. In general terms, with these units, the vapor-densities are equal to the equivalents divided by the combining volumes. Thus the densities of nitrogen, chlorine, iodine, bromine, &c., are simply represented by their equivalents 14, 35·5, 127, 80. The vapor-density of sulphur is $\frac{16}{\frac{1}{3}}=48$, that of oxygen $\frac{8}{\frac{1}{2}}=16$. It must be remembered that upon this system 2 vols. correspond to 4 vols. upon the scale in which the density of air =1. Hence the vapor-densities of nearly all organic compounds are simply half their equivalents, when all the hydrogen constants are taken as units. If it be objected to this system that experiment always compares the weights of equal volumes of air and vapor, it

is sufficient to reply that, in calculating the result of the experiment, the ratio of the densities of air and hydrogen may always be taken, so as not to increase the labor of calculation in the least. Would not, for all chemical purposes, a single uniform system of units be preferable to a mixed system such as that which we now employ?—W. G.]

2. *On the combinations of the hydracids with hydrocarburets.*—BERTHELOT finds that the hydrocarburets homologous with olefiant gas, under the influence of heat and pressure, unite with various hydracids so as to form the chlorids, bromids, &c. of the methyl series. Thus propylene, heated for seventy hours to 100° C. with an aqueous solution of chlorhydric acid, is completely absorbed, the reaction being expressed by the equation $C_6H_6+HCl=C_6H_7Cl$.

The chlorids, iodids and bromids of amylene, caprylene, ethylene and acetene may be formed by precisely similar processes, so that it is possible to pass from the hydrocarburets to the corresponding alcohols by simple and uniform processes.—*Comptes Rendus*, xliv, 1350.

3. *On inverse substitutions.*—The same chemist has also discovered new processes by which iodine, bromine, and chlorine in organic bodies may be replaced by hydrogen. After detailing the processes employed by other chemists in effecting the same object, the author describes his own methods, which consist either in employing free hydrogen at a high temperature, or in the concurrence of two simultaneous affinities equivalent to the use of nascent hydrogen. Free hydrogen unites with the chlorine of chlorids of carbon at a low red heat, the corresponding carburet of hydrogen being regenerated. A portion of the carburet is always destroyed at the same time. Thus we have the equations

$$C_4Cl_4+8H=C_4H_4+4HCl$$
$$C_4Cl_6+10H=C_4H_4+6HCl$$
$$C_{20}Cl_8+16H=C_{20}H_8+8HCl, \text{ &c. &c.}$$

When bromid of ethylene, $C_4H_4Br_2$, water and iodid of potassium were heated together, hydruret of ethyl was formed, while olefiant gas, carbonic acid and oxyd, and hydrogen were also set free. The temperature should be near 275° C. and the operation conducted slowly. The bromids of propylene and amylene, when heated to 275° C. with copper, water and iodid of potassium, yield propylene and amylene. By similar processes the author obtained marsh gas from chloroform, bromoform and iodoform. Finally bichlorhydrine, $C_6H_5Cl_3$, one of the chlorhydric ethers of glycerine, yields under similar circumstances propylene, C_6H_6, hydruret of propyl, C_6H_8, hydrogen and carbonic acid.—*Comptes Rendus*, xliv, 1246, 1349.

4. *On Silicium and the metallic siliciurets.*—DEVILLE and CARON have presented to the Academy a memoir upon silicium or silicon which exhibits many points of special interest. The authors find, in the first place, that aluminium is not the only metal which possesses the property of dissolving silicon, but that zinc may also be made to act advantageously as a solvent. The preparation of crystalline silicon by means of zinc is very simple and easy of execution. An earthen crucible is to be heated to redness and a carefully made mixture of three parts of fluosilicate of potassium, one part of sodium cut in small pieces, and one part of granulated zinc is to be thrown into it. The reaction ensuing is very feeble

and not sufficient to effect the fusion of the mass. The crucible must therefore be kept at a red heat until the scoria is completely fused. The heat must not be high enough to vaporize the zinc or the operation would be lost. After slow cooling, the crucible is to be broken, when a button of zinc will be found, penetrated through its whole mass, and especially on its upper surface, by long needles of silicon. These are groups of regular octahedrons imbedded in each other parallel to the axis which unites the summits of two opposite angles. To extract these crystals, it is only necessary to dissolve the zinc in chlorhydric acid, and then boil the silicon with nitric acid. In this way, crystallized silicon can be obtained in more beautiful crystals, and in larger quantity, than by any other method. The only portion of silicon lost in this process, is that disengaged in the form of siliciuret of hydrogen at the moment of the solution of the zinc. If the alloy of zinc and silicon be heated beyond the point at which the metal volatilizes, the silicon remains in the state of a fused mass which is entirely free from zinc. Pure silicon may be fused and run into moulds. In this manner the authors prepared ingots which were presented to the Academy. The authors are now engaged in studying the alloys of silicon which appear to be of much interest. The alloys with iron are very fusible and in their physical properties resemble cast iron and steel. A very hard, brittle and white alloy of silicon and copper containing twelve per cent of silicon is prepared by fusing together three parts of fluosilicate of potash, one part of sodium, and one part of copper turnings, till a very liquid scoria is obtained. An alloy of copper and silicon containing 4·8 per cent of silicon possesses a beautiful clear bronze color. It is a little less hard than iron and may be filed, sawed, or turned, like that metal. It is perfectly ductile, and wires drawn from it are as tenacious as those of iron. The hardness of the siliciurets increases with the quantity of silicon, but at the same time their ductility diminishes. They are all characterized by the fact that silicon is uniformly distributed throughout the mass so that the alloys are homogeneous and not susceptible of liquation. The authors presented to the Academy two small cannon made of alloys of copper and silicon. They furnish examples of what may be done in the arts by the application of the alkaline metals, and of the progress which is every day making in the manufacture of sodium. The authors have not limited their experiments to silicon, but expect by similar methods to prepare other simple or compound bodies in a crystallized state.—*Comptes Rendus*, xlv, 163, *Aug.* 1857.

5. *On the oil of the Dutch chemists.*—Wurtz has communicated to the Academy a note upon this often studied substance, showing it to be the chlorid of the glycol radical, C_4H_4, which exists in glycolic alcohol, and which is diatomic. The chlorid is easily prepared by the action of perchlorid of phosphorus upon glycol-alcohol, the reaction being represented by the equation

$$C_4H_6O_4 + 2PCl_5 = 2HCl + 2PO_2Cl_3 + C_4H_4Cl_2.$$

The Dutch liquid bears therefore the same relation to the glycol-radical which chlorid of ethyl does to ethyl, the former being diatomic and referable to the type of four equivalents of hydrogen, so that we have

$$\left.\begin{matrix} C_4H_4 \\ H_2 \end{matrix}\right\} = \left.\begin{matrix} C_4H_4 \\ Cl_2 \end{matrix}\right\} = \left.\begin{matrix} H_2 \\ H_2 \end{matrix}\right\}.$$

The author remarks that the Dutch liquid is entirely different in properties from the isomeric body, $C_4H_4Cl.Cl$, which is obtained by the action of chlorine upon chlorid of ethyl.—*Comptes Rendus*, xlv, 228.

6. *New derivatives of salicyl and new iodids of organic radicals.*—Cahours has studied the action of chlorid of acetyl (acetoxyl of Kolbe), as well as of other similar chlorids upon salicylic acid, setting out from the observation of Bertagnini that chlorid of acetyl by acting upon oil of bitter almonds produces cinnamic acid, according to the equation

$$C_{14}H_6O_2 + C_4H_3O_2Cl = HCl + C_{18}H_8O_4.$$

Hydruret of salicyl is strongly attacked by the chlorid of acetyl, but the product is not an acid but a neutral substance which has the formula $C_{18}H_8O_6$, and which is not decomposed by boiling with alkaline solutions, so that it is not identical with cinnamic acid. Benzoic acid under the same circumstances yields another isomeric body, namely, aceto-benzoic acid, which under the influence of water yields benzoic and acetic acids. The chlorids of cumyl, anisyl and succinyl behave in a similar manner toward the hydruret of salicyl, and yield new bodies which the author terms cumosalicyl, anisosalicyl and succinosalicyl. The author remarks, that in these compounds the elements are not merely in juxtaposition, as in the double anhydrous acids, but are fused together so that the distinctive characters of each are lost. A theory which satisfactorily accounts for these facts is still wanting. In conclusion the author describes the iodids of several of the acetyl radicals, which he obtained by the action of iodid of phosphorus upon butyrate, acetate and valerate of potash carefully dried. The iodid of acetyl (acetoxyl) is a heavy brown liquid boiling between 104° and 105° C. The iodid of butyryl boils at 146°; that of valeryl at 168°. They are perfectly analogous, in their chemical relations, to the corresponding bromids and chlorids.—*Comptes Rendus*, xliv, 1252.

[The iodid of acetyl has also been described in the Quarterly Journal of the Chemical Society by Guthrie, who obtained it by the action of iodid of phosphorus upon anhydrous acetic acid. It is very much to be regretted that the nomenclature of Kolbe is not generally adopted, so that the term acetyl may retain the signification originally given it by Berzelius and denote the radical C_4H_3, while acetoxyl is applied to the secondary radical $C_4H_3O_2$.—W. G.]

7. *On the true formula of oxalic acid.*—Wurtz has endeavored to show that oxalic acid is not only to be considered as bibasic, but that it bears the same relation to glycol-alcohol which acetic acid does to ordinary alcohol. When glycol is completely oxydized, as for example, by means of platinum black, carbonic acid is disengaged and the glycol disappears. When however the glycol is previously diluted with several times its volume of water, glycolic acid is formed. Nitric acid also, when acting slowly, oxydizes glycol, producing glycolic acid, the formula of which, it will be remembered, is $C_4H_4O_6$. By the more rapid action of nitric acid, oxalic acid is produced. The following formulas exhibit the relation between glycol and the two derived acids.

$$\left.\begin{matrix} C_4H_4 \\ H_2 \end{matrix}\right\} O_4, \qquad \left.\begin{matrix} C_4H_2O_2 \\ H_2 \end{matrix}\right\} O_4, \qquad \left.\begin{matrix} C_4O_4 \\ H_2 \end{matrix}\right\} O_4.$$

Glycol, Glycolic acid, Oxalic acid.

These relations are the same as those between alcohol and acetic acid, since we have

$$\left.\begin{matrix} C_4H_5 \\ H \end{matrix}\right\} O_2, \qquad \left.\begin{matrix} C_4H_3O_2 \\ H \end{matrix}\right\} O_2.$$

The result is in all cases due to a regular oxydation or rather substitution of oxygen for hydrogen. The author considers this result as proving that oxalic acid contains four equivalents of carbon, since it is derived from olefiant gas. The other acids which are homologous with oxalic acid must be derived in a precisely similar manner from the homologues of glycol. So that according to Wurtz we have the three series,

$C_2H_4O_4$ Methylglycol	$C_2H_2O_6$ Carbonic acid	———
$C_4H_6O_4$ Glycol	$C_4H_4O_6$ Glycolic acid	$C_4H_2O_8$ Oxalic acid
$C_6H_8O_4$ Propylglycol	$C_6H_6O_6$ Lactic acid	$C_6H_4O_8$ —— ——
$C_8H_{10}O_4$ Butylglycol	$C_8H_8O_8$ —— ——	$C_8H_6O_8$ Succinic acid
&c.	&c.	&c.

—*Comptes Rendus*, xliv, 1306.

[*Note.*—It may reasonably be objected to this view that with the formula $C_2H_2O_6$, carbonic acid must be bibasic. But the other acids, which, according to Wurtz, are homologous with it are monobasic. Can we have a series of homologous monobasic acids of which the first term is bibasic?—W. G.]

8. *Protosulphid of carbon.*—E. Baudrimont has discovered the sulphur compound of carbon which corresponds in constitution to carbonic oxyd. The author gives various processes for preparing this substance, the best of which consists in passing the vapor of bisulphid of carbon over spongy platinum or pumice stone heated to redness. An abundant deposit of sulphur is formed, while CS escapes as a gas. The new gas is colorless and has an odor which suggests that of the bisulphid, but which is strongly ethereal and not disagreeable. It burns with a blue flame, producing carbonic and sulphurous acids with a little sulphur. It is not liquefied by a mixture of ice and salt. Water dissolves its own volume but decomposes the gas into sulphid of hydrogen and oxyd of carbon, $HO+CS=CO+HS$. Alkaline solutions of potash and soda rapidly decompose it; thus with lime water we have the equation

$$CaO+CS=CaS+CO.$$

When the protosulphid of carbon is exposed to sunlight with an equal volume of chlorine there is a partial condensation and new products are found which the author is engaged in studying.—*Comptes Rendus*, xliv, 1000.

Persoz claims the discovery of this compound of carbon and sulphur, and refers in support of his assertion to his work on chemistry, published at Strasbourg in 1837–38.—*Comptes Rendus*, xliv, 1218.

9. *On Glyoxal and Glyoxylic acid.*—Debus has discovered among the products of the oxydation of alcohol by nitric acid, a new aldehyd which he terms *glyoxal*, and a new acid, *glyoxylic acid*. The constitution of glyoxal is represented by the formula $C_4H_2O_4$, and it is diatomic as is shown by its combinations with alkaline bisulphites. Thus the soda salt has the formula

$$C_4H_2O_4+2(NaO.SO_2+HO.SO_2)+HO.$$

Glyoxal may be obtained by decomposing the corresponding baryta salt by sulphuric acid; it affords an amorphous, transparent, faintly yellow mass which is readily soluble in water, alcohol, and ether. Its aqueous solution reduces silver from the nitrate: its solution in ether gives a white precipitate with ammonia. Caustic alkalies convert glyoxal into glycolic acid, even in the cold, and very easily on heating. Thus the reaction with lime is expressed by the equation

$$C_4H_2O_4 + CaO.HO = C_4H_3O_5, CaO.$$

Evaporated on a water-bath with a little very dilute nitric acid, glyoxal yields glyoxylic acid, $C_4H_2O_6$: much nitric acid converts this into oxalic acid, $C_4H_2O_8$. The author regards glyoxal as the aldehyd of the biacid alcohol glycol, $C_4H_6O_4 = C_4H_4O_2 + 2HO$. While however acetic aldehyd combines with only one equivalent of an alkaline bisulphite and yields on oxydation a monobasic acid, glyoxal combines with two equivalents of a bisulphite and yields on oxydation two bibasic acids. In conclusion the author points out the relations between glyoxal and certain organic acids.—*Ann. der Chemie u. Pharm.*, cii, 20.

10. *On the equivalents of Nickel and Cobalt.*—Schneider has determined the equivalents of nickel and cobalt by analyses of their oxalates, both carbon and metallic oxyd being directly determined. The author found for nickel as the mean of four experiments the equivalent 29·025, and for cobalt 30·003. These numbers may of course be taken as exactly 29 and 30, so that iron, nickel and cobalt form an ascending scale of equivalents, 28, 29, 30.—*Pogg. Ann.*, ci, 387. W. G.

II. GEOLOGY.

1. *Geological Report of the Midland Counties of North Carolina;* by E. Emmons, illustrated with engravings, pp. 350, 1856.—Besides other matter, this Report contains the results of Dr. Emmons's Survey of the Coal Fields of North Carolina and of the sandstones associated with them. Of these, which have attracted much attention in former years, the following brief account is presented from the new light which has been shed upon both the coal fields and the sandstones.

The range of sandstones is well known to extend from the north part of Massachusetts on Connecticut River southward to Long Island Sound; to begin again on the southern part of the Hudson and continue into New Jersey, and, pursuing a course west of south, to appear in the States of Pennsylvania, Virginia, and North Carolina even into the bounds of South Carolina, though with interruptions. At the north this series was formerly called the New Red Sandstone, supposed to be geologically next above the Coal Formation, and a part of the Triassic. On the continent of Europe, the Trias is divided into *three* parts, the upper called the Keuper, the middle the Muschelkalk, and the lower the Bunter Sandstein. The Muschelkalk is wanting in England and in our country; the other two are admitted in the geology of England, and Dr. Emmons maintains the existence of both in North Carolina and at the north, as the Upper and the Lower Sandstones, separated by their peculiar conglomerates and a partial unconformability. If this is sustained, the Richmond coal field, and also that of N. Carolina, must be placed, not in the Lias, or

Oolite series, but entirely below both. In the former relation, Prof. Wm. B. Rogers placed the coal and sandstone in view of all the evidence attained in 1843. This view Sir Charles Lyell considered to be confirmed by his own subsequent special examination, as he states in his Elementary Geology, sixth edition, p. 330.

But more extensive examinations and discoveries of fossils, animal and vegetable, have changed the character and value of the evidence and led to some changes of opinions. Professor O. Heer, of the Federal Polytechnic School, Zurich, who is distinguished for his knowledge of fossil plants and insects, has carefully examined Prof. Emmons's N. Carolina Report, as well as specimens, and forwarded his conclusions and corrections, which are now before me, and have great importance in determining the geological position of the coal and the sandstone. The fossils mentioned by Prof. Heer are referred to the page of the N. Carolina Report, and his opinion given in quotation marks.

Strangerites obliquus Em., p. 325; "good."

Acrostichites oblongus, Göpp. n. sp., p. 326.—This fern has been taken for Pecopteris Whitbyensis, but is properly removed to another genus by Dr. Emmons, because it is very different, says Prof. Heer in the "form of its sori," and "bears no resemblance to fig. 2 and 4 of plate 109 of Brongniart, nor to the figure given by Lindley," of P. Whitbyensis. "It is the neuration in particular which proves their difference; for the American plant presents a neuration which is reticulated, or at least its secondary nerves are tied together, while in the English plant the secondary nerves are digital and dichotomous, without being tied together laterally."

The *P. Whitbyensis* then is not in the Richmond nor the N. Carolina Coal Field and is no evidence of their geological position.

Pecopteris Carolinensis Em., p. 327, is "Gutbierea Carolinensis, Heer."

Pecopteris (*Aspidites*) *bullatus*, Bunbury, p. 328.—Prof. Heer says this fern "resembles so closely the Pecopteris Stuttgartiensis of Brongniart that I do not hesitate to regard them as identical. It cannot be comprehended how Bunbury has failed to recognize at least the great resemblance to the European species, so abundant in the Keuper, at Stuttgart, at Bâle, at Basière, &c."

Neuropteris linæfolia, Bunbury, p. 329.—"It is not a Neuropteris but rather a *Cyclopteris*, resembling the C. pachyrachis, Göpp. (Fossil Genera pl. 4, fig. 13 and 14 of the Lias.")

Pecopteris falcatus Em., p. 327.—"It is probably a Loccopteris. It greatly resembles Loccopteris germinans of Göppert (Fossil genera, pamphlet 1 and 2, pl. 6, fig. 8.")

Dictyocaulus striatus, n. g., Em., p. 293.—This fossil has an obvious resemblance to Noeggerathia, p. 357 of Lyell's El. Geol., Ed. 6th, 1857.

Pterozamites decussatus, Em., p. 330.—Of this Cycadea, Prof. Heer writes; "Although the species is represented but by a part of a leaf, this specimen resembles so much Pterozamites longifolius that I do not doubt of their identity, that is, Pterozamites longifolius, Braun." Later specimens led Prof. Emmons to the same correction.

Equisetum columnare, p. 334.—Of this Calamitacæ Prof. Heer observes; "Emmons has drawn but a poor specimen, which however appears to be this species." All will coincide in this opinion who know the fossil and look at the figure.

Calamites arenaceus, p. 334; "good."

Calamites disjunctus,, Em., p. 334, "does not seem to be other than the C. arenaceus." The future will decide.

"In conclusion," says Prof. Heer, "such species as *Pterozamites longifolius*, *Equisetum columnare*, and *Pecopteris Stuttgartiensis*, are characteristic of the Keuper or Marnes Irisées of Germany, of France, and of Switzerland; some others are like species that are found in Europe in the Keuper and the inferior Lias, but are specifically different. There is no species really Oolitic in Virginia and North Carolina."

Besides the evidence from the fossil Flora, which shows the age of the Richmond and N. Carolina coal fields, there is other proof from the fossil Fauna. These are the Thecodonts, found in Europe in the Bunter Sandstein, or in the Permian, and some others.

Messrs. Riley and Stutchbury first named a *Palæosaurus* and *Thecodontosaurus* from the Bristol conglomerate, England. (See Lyell's El. Geol., 6th ed., p. 358.) Analogous remains were detected by Mr. Lea of Philadelphia in the sandstone of Milford, Penn., which he named *Clepsisaurus Pennsylvanicus*. In the Deep River and Dan River Coal field, Dr. Emmons found analogous remains, which he named *Clepsisaurus Carolinensis*, being somewhat different from that described by Mr. Lea. Another was named *C. Leai*, Em., in honor of I. Lea, the discoverer of the first "Thecodont saurians in our country." To this was added Rutiodon Carolinensis, *Em.*, and one or two species of Palæosaurus, related to the Thecodonts of the Permian in Europe.

Still more remarkable is the discovery of a mammal in the coal series of Chatham, N. C., which is named *Dromatherium sylvestre*, Em., a new genus and species of a placental insectivore associated with the Thecodont saurians.

Another letter has been recently received from Zurich, in which it is stated by Mr. Marcou that Sir Charles Lyell, after a visit of some weeks at Zurich, and a full examination of the specimens of fossils, figures, and descriptions from the Richmond and North Carolina coal series, has come to the conclusion that this group is "Permian, or else is the Bunter sandstein;" that the "Dromatherium is the most ancient mammal yet discovered;" and that he has "changed the age of the red sandstone in his German edition of the Manual of Elementary Geology, now in the course of publication at Freiburgh," and that he will "make the same change in his next English and French editions of his Manual." C. D.

Oct. 6, 1857.

Additional Remarks by J. D. Dana.—In the determination of the exact age of this sandstone, the only rock in this country east of the Mississippi occurring between the Carboniferous and the Cretaceous, we cannot be too cautious in the use of evidence. One or two considerations are therefore here suggested. In the first place, the Fauna and Flora of America of this modern epoch, is represented in Europe, and quite strikingly, as has been shown, by the Fauna and Flora of the later tertiary of Europe. The life of corresponding ages in the two continents has thus been older in America than in Europe. This is one point to be well weighed. Again, in determining the age of a rock from its fossils, we should rather look to those which indicate the more recent period, than those which bear the

other way. This criterion would bring us right with regard to our own epoch, while by avoiding it, we might be able to prove that we in America are of the Tertiary age of the world. Now, as Mr. Redfield has shown, the fossil fishes,—the most characteristic species of any formation,—are but *half* heterocercal and come nearer to the Jurassic type than the Triassic. There is hence reason for the opinion notwithstanding the important evidence brought forward by Dr. Emmons, that the Lias period may be represented by the formation; and we may be nearest the truth if we regard the whole formation as corresponding to the Lias and the latter half of the Trias. The examinations by Mr. Heer accord with this conclusion. The European subdivisions of the Trias we should not look for on this continent, even if we had the whole of the formation, any more than the European subdivisions of the Devonian in the American Devonian. American geology is deeply interested in the decision of this question, and owes much to Prof. Emmons for all that he has done towards its elucidation.

2. *Illustrations of Surface Geology;* by Edward Hitchcock, LL.D., Professor of Geology and Natural Theology in Amherst College. 156 pp., 4to, with 12 plates of sections, maps and views. From the ninth volume of the Smithsonian Contributions to Knowledge.—The subjects here treated under the title of Surface Geology, are, terraces, elevated beaches, drift and the associated phenomena, erosion of the surface forming valleys, and the agents of change, running water, the sea, icebergs and glaciers. The author presents the results of a large amount of personal examination made mainly in New England, though to some extent also in other countries. The heights and positions of numeous terraces along the Connecticut valley and some of its tributaries, and of some on the Merrimack river, are given in the text, and the plates contain illustrations both by sections and neat maps. The great subject of terraces, a phenomenon that characterizes the whole breadth of the continent (except perhaps at the south,) and therefore one of the grandest in the science, is approached in the right method, and an important step is taken towards solving the great problem. It is impossible in a brief notice to give a satisfactory review of the facts brought out in this long and valuable paper. We mention a few of the deductions.

1. True unstratified drift never covers the terraces; it is covered by the terrace material, and is therefore of older date.

2. The successive "beaches" and "terraces" are made out of the drift material, with few exceptions, all by essentially similar operations.

3. The river valleys were excavated before the terrace epoch, for in the rocky gorges, as at Bellows Falls, drift scratches are found near the present water level; these gorges therefore were not closed at that time by rocky barriers.

4. The highest distinct river-terraces noticed by the author in this country are as follows: at Bellows Falls, on the Connecticut, 226 feet *above the river;* on the Deerfield, 236 feet; on Genesee river (New York State) at Mount Morris, 348 feet. Above these heights, there are other levels designated *beaches*, and the most distinct of these are stated to be from 800 to 1200 feet high; while others, less distinct, are mentioned as found at 1200 to 2600 feet in the White Mountains and elsewhere.

5. The number of terraces is usually larger and the height greater on the small than on large streams; there are seldom over *three* or *four* on the Connecticut; while on some of its tributaries, there are sometimes as many as *ten*.

6. The terraces on opposite sides of the same stream very often do not correspond in elevation.

7. The terraces, in general, slope with the stream. They are usually somewhat the highest about a gorge, as if the gorge had occasioned a higher level to the river floods, and therefore more elevated depositions. They show by the stratification that they are the result of water-action; though stranded ice, it is urged, may have aided in making the variety of terrace called *moraine-terrace*.

8. The beaches also must have been produced by water, and this water must have been the ocean. "Hence I feel sure," says the author, "from the facts which I have stated, that over the northern parts of this country, this body of water must have stood at least 2000 feet above the present sea level; and I might safely put it at 2500 feet; for up to that height I have found drift modified by water."

9. As a consequence of these conclusions, it is argued that the drift period was a period of submergence for the larger part of the continent, and perhaps nearly the whole, and since that period, the ocean has stood 2000 feet deep over New England.

10. The formation of the succession of beaches might have taken place by a gradual elevation and drainage of the continent, without pauses in the movement of elevation; yet it is possible that some such pauses may have taken place. Still, the author argues against the probability of such pauses, and finally against the possibility of pauses for river terraces in general, on the ground mainly of their unequal heights on opposite sides of the streams.

11. As regards North America, "we may as yet safely say, that there is no evidence of the existence of life in the seas that covered it during the period of unmodified drift; and indeed we might say the same of a considerable part of the period of the modified drift and alluvium."

12. Glaciers probably existed on the mountain summits that stood above the waters, as facts appear to show.

13. The apparent elevation of the continent may have been a consequence of an actual sinking of the bed of the ocean drawing away the waters from the land.*

14. The drift scratches and transportations of stone, gravel, etc., were produced mainly by icebergs during a period of continental submergence.

* It is stated here that Professor Dana has sustained this view. But in his paper on Terraces, he only suggests it as a point to be considered in the study of terraces and not as an opinion he held. He has often urged that any change of elevation in the water-line of a coast, may be a result of either one, or of both, of the two causes,—changes of level in the continental areas of the earth's crust or in its oceanic areas, the latter having nearly three times the surface of the former. He has treated the subject in a general way, and has stated that the *average* elevation of the land of the globe in the present epoch, above that in the Silurian may not be more than is a necessary result of the deepening of the ocean's bed. But he has no where attributed any particular case of apparent elevation to simply and solely the drawing away of the waters by a sinking of the ocean's bottom.

To appreciate the amount of evidence of various kinds urged in the memoir as well as the labor expended in the researches, recourse must be had to the original paper. The so-called beaches, at the various elevations, which with the drifted stones and scratches are regarded as the special proofs of the great submergences in the ocean, are described with care, and laid down on the charts.

The most important link in this evidence of submergence is still undiscovered. We know of many submergences in past time, for we have marine fossils in the strata; and when we come to the very latest of these continental submergences, we naturally enquire for those unmistakable sea-records, the marine shells. The coasts of various countries, to a great height, bear this decisive mark of elevations during the recent post-tertiary epoch,—Scandinavia, and other parts of Europe, Peru, Chili, Patagonia, Australia, etc. etc. And on this continent, fossiliferous beds assure us of an elevation of 500 feet or so along the St. Lawrence and Lake Champlain during the terrace period, and of still greater changes in the Arctic. We feel a degree of delight in such positive demonstration; there is actual exhilaration in the sight of such an accumulation of shells as occurs at Beauport (Canada). And at the same time we look upon an argument as to continental submergence with distrust when the sea-relics fail, especially if this failure is for the whole continent, an area of millions of square miles. It is fair to doubt, and disbelieve, until the sea-record in fossils somewhere comes to light; and it is reasonable to regard it as much against its ever appearing that the indefatigable researches of Prof. Hitchcock have not found it. As to the supposed absence of life in the seas during the post-tertiary, can it be believed, when 25 to 40 per cent of the species of the preceding tertiary period are animals now living in our waters?

It is true, that if the supposed submergence had taken place, sea-shells are not to be looked for everywhere. In a river like the St. Lawrence, the water at Montreal is essentially on a level with the ocean, the elevation being but 21 feet, and not exceeding what is made by the flow of the water. Now notwithstanding this one level for the whole, the salt water—salt *enough to grow marine shells*—sets back in this great stream but a small part of this distance; and consequently while there are deposits with marine shells along the gulf, there are alluvial deposits without marine shells over the banks for 300 miles below Montreal. Now, when the sea stood 500 feet above its present water line, in the St. Lawrence, the pure salt water set back to Montreal and even as far as Ogdensburg, and also up Lake Champlain, where in either region it had its marine shells in profusion. But beyond this to the westward, the stream would, from some point, owing to the descending floods of fresh-water, be fresh, although on a level with the sea; and thus its shore flats—afterwards to become terraces—would have had no salt-water shells. Thus there might be true lake terraces and marine terraces made on the borders of the same waters, and all at the same level. In the Connecticut and Hudson rivers, again, sea-shells hardly enter the mouth. The lesson we learn is this: that in no case could we expect to find marine fossils in river terraces except where those rivers opened into wide estuaries;—and also, wherever wide estuaries existed, or where there was a shallow muddy sea-bottom off an existing coast, deposits of shells like those of the St. Lawrence and Lake

Champlain are to be looked for; and at least, in every thousand miles of such sea coast, some estuary or shore deposits might be reasonably expected.

While therefore, this important kind of evidence is wanting over this continent; and while it is not shown that icebergs could make the scorings of the rocks; and while it is not clear that a submergence of the northern part of North America could be a period of *unusual* glacier cold for the world when the effect of submerging northern lands it is known would mitigate cold, we have some reason for doubting the supposed submergence of the continent in the drift period.

This subject should be considered from every point of view. While constrained to differ in some few points from the memoir of Prof. Hitchcock we commend it as abounding in valuable observations. J. D. D.

3. *Outlines of the Continents.*—At the meeting of the American Association at Montreal, Prof. Peirce pointed out a relation between the direction of the coast-lines of the continents and the Arctic and Antarctic circles. The coast-lines, according to the view, lie in great circles that are tangents to these polar circles. Consequently if either pole of a terrestrial globe be elevated 23° 28′ so that the horizon becomes such a tangent great circle, and then the globe be slowly revolved, these coast-lines, as they pass the horizon circle, will be found approximately to coincide with it in direction. Elevating the north pole, and revolving in one direction, eastern North America makes this coincidence, and in the other direction western North America; and so for other great outlines of the land. He regarded this remarkable relation (23° 28′ being the obliquity of the ecliptic) as evidence that the sun by influencing in some way the cooling of the crust, had determined the grand outlines of the globe. We wait for a full announcement of his views before giving further details.

These facts had been announced in Boston journals a few months previous. Shortly after their first appearance in print, the work of Professor R. Owen of Kentucky, entitled "Key to the Geology of the Globe," was published in New York City—a work noticed in the number of this Journal for last July (this volume, p. 158); and it contains on page 36 a mention of the same relation and a similar elucidation by reference to a globe. Prof. Peirce alluded to this in conversation with us at Montreal. In the use of the observation the views of this work diverge from those of Prof. Peirce.

4. *Third Annual Report on the Geological Survey of the State of New Jersey for the year* 1856. 80 pp., 8vo. Trenton, 1857.—The first 38 pages of this pamphlet are occupied by the report of the geologist, William Kitchell, the next 30 with the report for the year on the geological survey of the southern division of the State by George H. Cook, Assistant State Geologist, and the remainder by a brief report of the State Topographical Engineer, E. L. Viele. The magnetic iron ores and iron furnaces of New Jersey are the subject of the first part of the volume, and many facts are brought out of practical value to the state. Prof. Cook, besides other results of importance, gives several analyses of the greensand and marls of New Jersey, which we propose to cite in a future number of this Journal.

The people of New Jersey have much to gain from the prosecution of the geological survey which has been begun, and we trust that it will not fail of the requisite support.

5. *Microgeology of Ehrenberg.*—A continuation of Ehrenberg's great work has been recently issued, consisting of 88 pages large folio; and it relates exclusively to North America. It consists of descriptions of earths and river sediments, from the different states of the country, as regards their infusorial contents, and tables of the results for each. The parcels examined and here described amount to 247, 85 of which are from Texas, 4 from Arkansas, 36 from the Washita and Neosho, etc. The number of microscopic species observed by Ehrenberg and Bailey in the Southern United States is 855; of these 148 are brackish water and marine species, about half of them being fossil and half living.

6. *Note on New Fossils in the Potsdam Sandstone at Keeseville, New York;* by J. D. Dana.—Hitherto the Potsdam Sandstone of New York, the lowest rock of the Silurian, has been known to afford no fossils but one or two species of the genus Lingula. Through the researches of a young and energetic student in geology, of New Haven, Mr. Frank H. Bradley, who visited Keeseville last summer and has recently been again at the locality, a species of Trilobite (genus *Calymene*) has been discovered, and also one of *Pleurotomaria*, besides an impression of a crinoidal disk. The Pleurotomaria is only a cast. The Trilobite, although a small one, its breadth but one-eighth of an inch, is well preserved. The buckler and caudal extremity have not been found together, but the markings of each are very distinct.—*Proceedings Montreal Meeting Amer. Assoc.*

III. BOTANY AND ZOOLOGY.

1. *An Elementary Course of Botany; Structural, Physiological, and Systematic; with a brief Outline of the Geographical and Geological Distribution of Plants.* By Arthur Henfrey, F.R.S., L.S., &c., Professor of Botany in King's College, London, etc. 1857, pp. 702, small 12mo. London, Van Voorst.—This is a well-planned, compact, and comprehensive work, in which we may say, that the author has fairly accomplished his purpose, namely:—"to produce a good working text-book for the student, from which may be obtained a groundwork of knowledge in all branches of the science, without the attention being diverted from the more striking features of the subject by details comparatively unimportant."

The work is divided into four parts: I. Morphology or Comparative Anatomy, treating, in successive chapters, 1st. of General Morphology; 2nd, of the Morphology of the Phanerogamia, or the parts of Flowering plants and their modifications, and the laws which regulate them; 3d, Morphology of the Cryptogamia. Part II. Systematic Botany; treating 1st, of the principles of Classification; 2nd, of systems of Classification, and 3d, systematic descriptions of the Natural Orders, followed by an artificial analysis. Part III. Physiology; comprising, 1st, the physiological anatomy of plants; 2d, general considerations on the physiology of plants; 3d, physiology of vegetation; 4th, the reproduction of plants; 5th, Miscellaneous phenomena, under which are ranked the evolution of heat in plants, luminosity, and movements of plants. Part IV. Geographical and Geological Botany, very summarily disposed of in about forty pages.

It seems strange at first to interpose Systematic Botany between the morphological and the physiological; but if the anatomy and physiology of plants are to be completely disjoined from the study of the organs of the plant as a whole, the present arrangement is perhaps as good as any. It is adopted, as the preface shows, for the convenience of instructing medical students, who compose the principal part of classes in Great Britain as well as on the Continent;—for whom "one short course of lectures is devoted to this science, and three months is commonly all the time allotted to the teacher for laying the foundations and building the superstructure of a knowledge of botany in the minds of his pupils, very few of whom come prepared even with the most rudimentary acquaintance with the science." But the author remarks that "if the previous education of medical students prepared them, as it should, with an elementary knowledge of the natural sciences, we should make physiology the most conspicuous feature of a course of botany in a medical school."

While in England botany is scarcely an academical study, here it pertains to collegiate and academical instruction where it is taught at all. In Europe not even an apothecary can be licensed without passing an examination in botany; in the whole United States, we believe, it forms no part, at least no regular part, of the medical curriculum; no medical school has a botanical chair; and no knowledge whatever of the science of the vegetable kingdom, which supplies the greater part of the *materia medica*, is required for the degree of Doctor in Medicine!

Professor Henfrey is chiefly known, and most highly esteemed, as a vegetable anatomist. Upon this subject he may speak with an authority which, as a systematist, or even as a morphologist, he would not pretend to. We shall offer no apology, therefore, for making an occasional criticism, and for pointing out several errors in matters of detail. These are not intended to disparage the work, for if we had not formed a high opinion of it on the whole, we should not take this trouble.

As respects the first point noticed, our author, if wrong, is not alone. Still, we hardly expected him to teach that the radicle of the embryo is the true root; and we cannot let pass unchallenged his reiterated statement that in Monocotyledons, the radicle, or its inferior extremity, is never developed into a root in germination, but is abortive (pp. 14, 16, 18, 391, 537). Any one who will examine the germination of the seed of an Iris, an Onion, or even of a grain of Indian corn, cannot fail to perceive that a primary root is developed, and that this is a direct prolongation of the extremity of the radicle. This, indeed, does not continue as a tap-root; neither does it in a great many Dicotyledons. In squashes, pumpkins, &c., there is no one primary root, but a cluster of rootlets from the first, all springing from the base of the stout radicle. In fact, this distinction between Monocotyledons and Dicotyledons is null. A character of certain monocotyledonous embryos, neither strictly peculiar to the class, nor by any means universal in it, should not be assumed as distinctive. As to the morphology of the radicle itself, we suppose that the germination of any of the larger Cucurbitaceæ, or of a bean, would suffice to convince any observer that the radicle is simply the first internode of the stem, giving birth to the primary root from its inferior extremity, usually,—and indeed, from the exceptional cases where it does not we should draw addi-

tional proof of its cauline nature. In fact, we know of no character in which a root differs from an internode of a stem in which it does not also differ from the radicle, excepting its tendency to direct its inferior extremity downwards. Again, should the statement, that "the radicle of a monocotyledonous embryo is never developed" be held to mean that the radicle never lengthens, we remark, no more does it in the pea and some other hypogæous Dicotyledons; and we are not quite sure that the statement is absolutely true of all Monocotyledons.

Root-hairs or *fibrillæ* are mentioned (p. 19) as "often" occurring on young roots. Do they not *always* occur? Surely it cannot be true that: "the branches of the axial root are originally growths from the apex of the root thrown off to the side," (p. 538). By some slip of the pen, *Myrica Gale*, is adduced as an instance of whorled leaves (p. 45).

On p. 49 the expression "*over* the petiole" instead of *above* or within it, would lead to a misconception.

Something more might be said about the tendrils of Cucurbitaceæ (which besides are not always single); but are the students of King's College really taught that, "tendrils of the vine are metamorphosed flowering branches *arising in the axils of the leaves?*" (p. 62.)

"In all seeds except in those of the few orders which present an incomplete or acotyledonous embryo" we do *not* find the young plant possessed * * of a plumule" (p. 66). Even some much developed embryos, such as those of Maple and Morning Glory do not show the plumule until after the full development of the cotyledons. It may be said, indeed, that the plumule is *in posse* when not *in esse*, but so it is no less in the cases excepted from the statement.

Very singular is the statement (on p. 68) that in England "the terminal bud of the Lilac is generally killed by the frost in the winter;" since in our much colder winter it is as completely hardy as the other buds whenever it happens to be formed, and, like them, is well developed before summer is over. As a general rule here, and we presume in England also, no terminal winter bud appears during the growing season, and so there is none to be killed by the frost of the following winter.

The deeply alveolate receptacle of the Cotton-Thistle is figured (on p. 78) as an illustration of a *paleaceous* receptacle.

Truly *terminal* flowers are said to be rare (p. 86): we do not quite understand this.

The interesting questions relating to the phyllotaxy and symmetry of the flower are clearly stated, but no new light is brought to bear upon them,—nor all of the old. The opposition of the stamens of Rhamnaceæ to the petals is, as usual, attributed to the probable suppression of an outer stamineal circle, although there is nothing in the blossom (as there is in Geraniaceæ, &c.) to base the supposition upon. And our author has overlooked the most natural of explanations for this and strictly like cases, the one moreover which tells directly against the doctrine of transverse chorisis.—viz., that in these cases of *ante-position* there is a return to normal phyllotaxy, i. e. to the superposition of the corresponding elements of successive whorls,—a view first suggested, we believe, by Lestibudois.

"Real cases of *collateral* multiplication may probably be explained by comparison of a primary staminal leaf with an ordinary compound stem-

leaf, and supposing the filament to subdivide like the petiole does [sic] in such cases." This is certainly the way we regard it; and as respects the application of this hypothesis to the stamens of Cruciferæ, we do not see what argument *Megacarpæa polyandra* brings against it; as the increase in the number of stamens is quite as explicable upon this as upon the ordinary theory. Indeed our author's view that the glands represent suppressed stamens would seem to be negatived by this very case, since the glands have not disappeared with the increase of the stamens, but the contrary.

The abnormal fertile flowers of *Viola* and *Impatiens* are not "achlamydeous," as our author states them to be (p. 90): generally they are not even apetalous.

In the botanical sense of the word, and as it is employed in the same sentence (p. 93) the petals of the vine cannot be said "to cohere above." The valvate petals are merely caducous for the most part before expanding, just as is more decidedly the case in many Araliaceæ. In passing, we remark that a valvate æstivation of the corolla in the latter is much less distinctive than our author supposes (p. 311). *Aralia* itself has the petals imbricated in the bud.

It is becoming common to regard the tube of a so-called *superior* calyx as a cup-like receptacle; and there appears to be reason for it in Cactaceæ and some other cases. Prof. Henfrey would seem to apply this view universally; "for example, in Rosaceæ, Umbelliferæ, Cucurbitaceæ, Compositæ" [!] &c. But if applied to *Rosa*, why not to the Sanguisorbeæ and to other Rosaceæ with a calyx-tube lined with a disk bearing the stamens, &c.? And is the cup a receptacle in those Melastomaceæ which have an adnate ovary, but a calyx when the ovary is free? And how is it when the ovary and cup cohere only by the nerves of the latter?

For *paleæ* Prof. Henfrey coins an English word *pales* (p. 110), of which the singular would probably be *pale*. We would propose to call them *palets*.

There are convincing reasons why the perigynium of Carex cannot be regarded as a perianth, as our author takes it to be (p. 111).

It is not correct to say that the false dissepiments of Datura are formed "while the seeds are ripening" (p. 124); they equally exist in the ovary. And we doubt if the transverse false septa in *Cathartocarpus* and other Leguminosæ are "placental developments."

We are pleased to find that our author prefers to consider placentæ as belonging to the carpels rather than to the axis, although the close of paragraph 226 appears to imply the contrary.

We cannot agree that, "externally the campylotropous ovule resembles the anatropous, except that there is no rhaphe," (p. 130). No attentive student could fail to recognize the difference, especially in the families cited (Cruciferæ and Caryophyllaceæ).

Ripening must be regarded in a remarkably broad sense when it is stated with emphasis, "that the distinction between endocarps and epicarps, in the common stone-fruits, arises entirely during the ripening of fruit." Also: "it is well known that the easy separation of the pulp from the stone is a sign of ripeness." When are cling-stone peaches ripe? Again: "In Taxus * * during the *ripening* of the seed a succu-

lent cup-like envelope grows up around it" (p. 136). Is ripening synonymous with the formation and growth, as well as the maturing of the fruit?

Lindley's system of the classification and nomenclature of fruits is adopted, with some modifications. It is well to have such a system, as an analysis of the diversities of structure; but of the thirty-six kinds so carefully defined and named only fifteen or sixteen are ever used in descriptive botany, or ever will be, it is devoutly hoped. There is much inconvenience in practice, and little advantage in designating every possible modification of the same organ or set of organs by a distinct substantive name, or in distinguishing by separate technical names fruits formed of a simple ovary from those of a compound ovary, or fruits with an adherent from those with a free ovary. Why not call the gooseberry and the grape equally a berry, instead of restricting this name to the former and naming the latter a *nuculanium;* and why name the pod of an Iris a *diplotegia*, while that of a Lily is called a *capsule?* And while we term the pod of *Saxifraga stellaris* a *capsule*, and that of *S. tridactylites* a *diplotegia*, what name are we to apply to that of *S. aizoides*, which is only half-superior?

Probably a wrong example is adduced on p. 148, for we cannot believe that any species of Ranunculus has the rhaphe averse from the placenta in the ripe fruit. By an oversight, on the same page, the fruits of Labiatæ are spoken of as seeds.

As respects the systematic part, the chapters on the principles of classification, nomenclature, &c., strike us as sound and good throughout; and in the account of the natural orders a great amount of information, such as the medical student needs, is given in a comparatively small space. Errors or misconceptions will necessarily occur in the compilation of such an amount of materials, treating of structure, affinities, distribution, sensible properties and medicinal or economical uses. They are not more numerous than was to be expected, and we are not disposed to make them the subject of criticism.

We may remark, in passing, that, as respects the morphology of the androecium in Fumariaceæ, the name of the writer of the present notice is referred to, by some misconception, as adopting Lindley's well-known hypothesis of the splitting of two stamens into halves; whereas he has maintained a very different view. And then this is mentioned as "offering a phenomenon of chorisis," which in that view is quite incomprehensible to us.

We were surprised at the statement that the bark and leaves of *Hamamelis Virginica* "are astringent and contain an *acrid volatile oil*," p. 307. We trace it back to Lindley's *Vegetable kingdom*, p. 784, and find: "The kernels of *Hamamelis Virginica* are oily and eatable. The leaves and bark are very astringent, and also contain a peculiar acrid essential oil;" and this, we find, comes from Endlicher's *Enchiridion*. How did this bland and inert plant acquire such a reputation? Dr. Barton, who has figured it, says nothing of its possessing any sensible properties or useful qualities at all, except its use for divining-rods; nor do Pursh, Bigelow, Elliott, Darlington, &c., allude to any popular reputation of such qualities. No sign of any essential oil is to be detected in the foliage, and prolonged

mastication of the leaves and bark while we write yields not the slightest trace of acridity and hardly any of astringency; no more, certainly, than a Beech-leaf. We never heard of the seeds being eaten; and as they are "about the size of a grain of barley," or not much larger, and have a thick bony coat, they are not likely to become an important article of diet. After some search, we find the source of these extraordinary statements in the *Medical Flora* of the excentric Rafinesque. He says the seeds are called Pistachio nuts in the Southern States, are rather oily and palatable, &c., but he neglects to mention their size. He adds, "the bark and leaves are somewhat bitter, very astringent, leaving a sweetish pungent taste. The smell is not unpleasant. It has not been analyzed as yet, but probably contains tannin, amarine, extractive, and an essential oil." To all this, Endlicher, on the strength of "the sweetish pungent taste," has added the acridity; and so one of the blandest and most useless of shrubs gets a world-wide and wholly factitious reputation for active medical qualities and esculent seeds; and even Dr. Griffith, who must have known the shrub, has been induced to give it a place in his Medical Botany.

Our remaining remark relates to the random way in which mere analogies are mixed up with affinities in estimating or expressing the relationship of orders, &c., in this as in some other more notable works. It is, or at least ought to be, well understood, that mere *analogy*, i. e., likeness in some one respect only, however striking the imitation, is no indication of relationship, but that relationship rests upon *affinity*, i. e., upon agreement or similarity in the whole plan of structure, and especially of floral structure, whether general or particular, as the case may be. To speak, therefore, of 'evident' and 'most distinct' affinities between *Coniferæ* and *Lycopodiaceæ* is an example of this prevalent misconception of what affinity is. This is more intelligible, however, than the 'approach' suggested of *Aquifoliaceæ* to *Loganiaceæ* and *Apocynaceæ*, while their resemblance to *Celastraceæ* is thought to be of small account; or that of *Umbelliferæ* to *Rubiaceæ*, *Saxifragaceæ*, and even to *Geraniaceæ*, to which the resemblances do indeed "seem rather superficial." Again, *Xanthoxylaceæ* (i. e. *Rutaceæ*) are said to have considerable affinity to *Oleaceæ*, because *Ptelea*, in the former, has a samaroid fruit, as has *Fraxinus* in the latter. May we add, as quite as much to the purpose, that the common Xanthoxylums have pinnate leaves, and are popularly called Prickly Ash?

The study of affinities is neither guess-work nor divination, but a matter of logical deduction from structure, based upon scientific principles,—principles recognized and acted upon by sound botanists with considerable unanimity, although they have never been reduced to a system, nor expounded in detail, so as to make them matters of elementary instruction. Until this desideratum is supplied, the young botanist can do no better than to take as models the writings of Brown, and of those botanists who, according to their ability, have most closely followed the footsteps of this master in science.

Having continued this review far beyond our intention at the outset, we have small space left for noticing the best part of Prof. Henfrey's treatise, namely, the third or Physiological part. Suffice it to say that, in the important chapter on the physiological anatomy of plants, our author writes

from the fullness of his acquaintance with the writings and doings of all the continental phytotomists, and also with the authority of an experienced original investigator. And, so far as we know, it comprises much the best *resumé* of vegetable anatomy and development now extant in the English language, at once succinct, clear, trustworthy, and well brought up to the present state of the science. Perhaps the succeeding chapters, on the Physiology of Plants generally, the Physiology of Vegetation, and on Reproduction, are equally commendable in their way; but we have as yet barely glanced over the pages. We like the following definition, and the ensuing paragraph upon the *rôle* of vitality in plants.

"The physiology of plants is that department of botany in which we investigate the phenomena of the *life* of plants, manifested in a series of changes taking place in the diverse parts of which each plant is composed."—p. 475.

"The physiological phenomena which indicate vitality are always of more or less complex nature, and admit of being analyzed into a number of factors, of which a large proportion are found to be purely physical or chemical. A very considerable part of the changes which accompany the process of organization are the results of the action of physical and chemical forces, [and] capable of being explained up to a certain point, by the known laws of those forces. But in every case, after referring all the chemical and physical phenomena to their respective places, there remains a residual phenomenon to be accounted for, which is precisely the most important of all,—namely, that in living organic structures . . . . the laws of inorganic matter are subdued under a higher influence, and caused to undergo modifications never occurring except in the presence of living matter; while—most important of all—the peculiar compounds of matter thus produced are not only made to assume forms, according to definite laws, totally unlike any forms of mineral matter, but [to] constitute bodies manifesting a continued interchange of material with the surrounding media, which, instead of resulting in decomposition, as in mineral bodies, effects a reproduction and increase of the already existing [organized] matter."—p. 542.

In the paragraph on the longevity of trees (p. 549), we find renewed occasion to notice the longevity of unfounded statements, copied from one book into another long after the error has been pointed out. Here again the *Adansonia* of Senegal and the *Wellingtonia* or *Sequoia* of California figure as trees "whose age, deduced from the rings of growth of the stems would amount to upwards of 3000 years." There is really no evidence to prove that the famous Baobabs described by Adanson are of such an age, and as to the *Wellingtonia* in question, an actual counting of the rings has shown that the tree was not half so old as it was vaguely computed to be.

The chapter on Reproduction appears to be excellent, as indeed we should expect. The geographical and geological part is necessarily very briefly treated. A. G.

2. *Naudin's Researches into the Specific Characters and the Varieties of the Genus Cucurbita*, are published in the 6th volume (4th series) of the *Annales des Sciences Naturelles*, and are of no small interest, being founded upon a very conscientious investigation of nearly all the known

forms, collected for the purpose, and cultivated under the author's eye at the *Jardin des Plantes*. These forms our author reduces to six species, and the alimentary sorts in cultivation, to three, viz. *Cucurbita maxima*, *C. Pepo*, and *C. moschata*. The remaining three species are *C. melanosperma* of Braun, newly introduced from Eastern Asia, and the two perennial and tuberous-rooted species, *C. perennis* and *C. digitata*, Gray, natives of our southwestern borders, the fruits of which are not esculent. Indeed the pumpkins and squashes cultivated in Northern Europe and with us, as now understood, belong to only two species, since the third, *C. moschata* hardly comes to perfection north of the Mediterranean region. Of these *C. maxima* is made to include *C. Melopepo;* and *C. Pepo*, comprising our pumpkins and a large part of our squashes, is made to include *C. ovifera*, *aurantia*, *verrucosa*, &c., and the species are defined by botanical characters, which apparently may be relied upon. The varieties of *C. maxima* fall into two main groups, characterized by their fruits, viz., the *Turbans*, having *crowned fruits*, that is, the summit projecting beyond the adnate calyx-tube, a peculiarity found in no other species, and the *crownless* sorts, in which this peculiarity is not manifest. The innumerable varieties of *C. Pepo* are arranged in seven groups, according to the configuration of their fruits.

M. Naudin has not undertaken to discuss the questions respecting the birth-place of these plants. He remarks that *C. maxima* and *C. moschata* have been known in European gardens scarcely above two centuries; but that *C. Pepo* was perhaps known to the Greeks and the Romans in the time of Pliny.

The younger DeCandolle, in his discussion of the history and origin of the principal cultivated plants, which form a most interesting chapter of his *Géographie Botanique*, although he is unable to assign them to any country as their home, confidently (perhaps too confidently) refers all the squashes and pumpkins to the Old World; but not to India, because they have no Sanscrit name. He will not believe that any of them came from America, and appears to think little of the current statements that squashes or pumpkins were in cultivation by our aborigines before the European settlement of the country. On the other hand, our lamented Dr. Harris, —who, during the later years of his life, assiduously studied this question, and who was very cautious in drawing conclusions,—had become satisfied that the North American Indians as far north even as to Canada, cultivated squashes and pumpkins, one or both, along with their maize, before the whites were established here. We are unable at this moment to refer to his manuscripts, or to what he had too imperfectly published upon this subject. But we well remember his laying much stress upon the narrative of Champlain; and with good reason, as it appears to us on turning casually to the pages of *Les Voyages du Sieur de Champlain .... ou Journal tres-fidèle des Observations faites et Decouvertes de la Nouvelle France*, &c. &c., edition of Jean Berjon, Paris, 1613, 4to.: also *Voyages et decouvertes faites en la Nouvelle France depuis l'année* 1615, *jusques à la fin de l'année* 1618,—second edition, published by Collet, in 1627, small 12mo,—to which volumes we desire to direct M. DeCandolle's attention. In Champlain's narrative of his own voyage along the coast of what is now the State of Maine, in the year 1604, and the two voyages

of Le Sieur de Mons along the coast of New England in 1605 and 1606, *Citrouilles* and *Courges* are repeatedly mentioned, along with maize (*Bled d'Inde*) and beans: e. g.

"Nous y vismes force *citrouilles, courges* & petum, qu'ils cultiuet aussi. . . . Pour les febues elles cõmẽçoiẽt à entrer en fleur, cõme faysoyẽt les courges et citrouilles."—p. 68.

"Ceux que nous auions enuoyes deuers eux, nous apporterent des petites citrouilles de la grosseur du poing, que nous mangeasmes en sallade comme coucombres, qui sont tresbonnes."—p. 77.

See also pp. 83, 115, 116. Of course it does not follow that these esculents were natives of New England, any more than maize: but both may probably have been carried northward together. Whatever their origin, our Indians were found cultivating them together at this early date as well as in later times. According to Nuttall the Indians along the whole Upper Missouri half a century ago were cultivating *Cucurbita verrucosa*. This common squash is, according to Naudin, a variety of *C. Pepo*, as also is *C. aurantia* (the *C. Texana* vel *ovifera*, Gray, Pl. Lindh.) which has every appearance of being indigenous in the western part of Texas, on the Rio Colorado and its upper tributaries. At least, this is the opinion of Mr. Lindheimer and of Mr. Charles Wright, two good judges. The latter personally informs us that, from the stations and localities in which alone it is met with, he could not suspect it to be other than an indigenous plant.

That the later Greeks and Romans possessed the bottle gourd or *Lagenaria*, and also some kind of summer squash, seems pretty clear; but we see no decisive reason for the opinion that they had any form of *Cucurbita Pepo*, as that species is now understood. According to DeCandolle, the earliest figures referable to this species are, one of *C. ovifera* by Lobel in 1576, and one of *C. verrucosa* by Dalechamp in 1587, namely, about a century after the discovery of America, and long after maize had become well known in the south of Europe: and we have seen that some forms probably of this very species (undoubtedly originating in a warmer region) had by this time found their way in this country nearly as far north as the climate will permit of their cultivation. So that there appears to be about the same evidence for the American origin of *some* squashes and of pumpkins that there is for the American origin of maize.

A remaining argument brought by DeCandolle against this view may also be turned the other way, namely, that no certain species of the genus is known as indigenous to America. He has equally allowed that none is known to be indigenous to the Old World. Now of the six species recognized by Naudin, two only are known in their natural wild state, and these are our southwestern species with perennial roots, viz., *C. perennis* and *C. digitata;* to which we add that *C. Pepo* itself (i. e. *C. ovifera* or *aurantia*) grows wild in the same district with *C. perennis*, and has the same appearance of being indigenous there. We leave the subject with these incidental remarks, as we did not intend here to investigate this question, and will briefly allude to another subject, upon which Naudin's investigations have thrown new light.

It is generally thought that the cultivated *Cucurbitaceæ*, and especially that the species of *Cucurbita*, cross-breed with extreme facility. Accord-

ing to Naudin this is true of the *races* only *inter se*. A good illustration of the immediate and great variation from this cause in the fruit of *C. Pepo* is given in Naudin's third plate, where fifteen different forms of the fruit are figured, taken from as many individual plants raised from seeds of one fruit, which had grown in the vicinity of other varieties. It is by no means certain, however, that all these forms originated from direct crossing. But the species themselves strangely refuse to hybridize. Naudin carefully experimented with the five species in cultivation at the *Jardin des Plantes* (viz., all known, except *C. digitata*): and out of seventy distinct trials all but five were utterly ineffectual. In five instances the fruit set, indeed, but in none of these was a single seed containing the vestige of an embryo produced! What are we to think, then, of the universal belief that squashes are spoiled by pumpkins grown in their vicinity, or pumpkins by squashes; and even melons (which are of a different genus) by squashes? The fact of some such influence seems to be well authenticated. Dr. Darlington, one of the most trustworthy of observers, speaks of it from his own knowledge, thus:—"When growing in the vicinity of squashes the fruit [of the pumpkin] is liable to be converted into a kind of hybrid, of little or no value. I have had a crop of pumpkins totally spoiled by that cause, the fruit becoming very hard and warty, unfit for the table and unsafe to give to cattle."—(*Fl. Cestr.*, ed. 2, p. 555.)

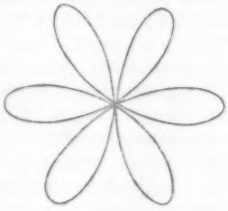
Now that this is not the effect of hybridation is clear from the fact that the result appears in the fruit of the season, not in that of the next year, viz., in a generation originated by the crossing. A clue is perhaps furnished by Naudin's observations, that the ovary is apt to set and even develop into a fruit in consequence of the application of the pollen of another species, although, as the result proves, none of the ovules are fertilized. And he hazards the conjecture that the pollen may exert a specific influence first upon the ovary, inciting its farther development, and then upon the ovules. To test this conjecture he was to examine the action, if any there be, of the pollen of *Cucurbita* upon the ovary of melons. The past summer,—which has been as unusually warm in Western Europe as it has been cool in this country,—must have favored such researches in Paris; and we may expect soon to hear of the result. Improbable as such an influence seems to be, it is hardly more so than the now authenticated fact that the graft of a variegated variety of a shrub or tree will slowly infect the stock, so that the variegation will at length break out in the foliage of the natural branches;—an old observation, which, according to the Gardener's Chronicle, has recently been verified in several instances. A. G.

IV. MISCELLANEOUS SCIENTIFIC INTELLIGENCE.

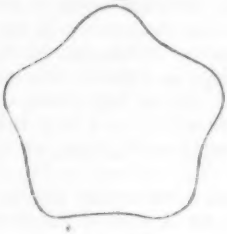
1. *Studies in Organic Morphology;* an Abstract of Lectures delivered before the Pottsville Scientific Association in 1855 and 1856, containing historical notes of this branch of science, together with original formulæ and constructions of curved lines, designed to assist in the imitation of organic forms; by JOHN WARREN. 58 pp. 8vo. Philadelphia, 1857.

J. B. Lippincott & Co.—This pamphlet relates to a branch of mathematics upon which little has hitherto been published in this country, and although the views are not new in principle, they are in part the result of original research by the author and new in some of their applications. The subject is the method of constructing curves of complex but regular figures similar to those in the Plant and Animal Kingdoms. The plates of numerous figures at the close of the pamphlet are instructive even to one who is not familiar with the use of the formulas on which they are constructed. The following are a few examples, from the 100 figures given.

1.



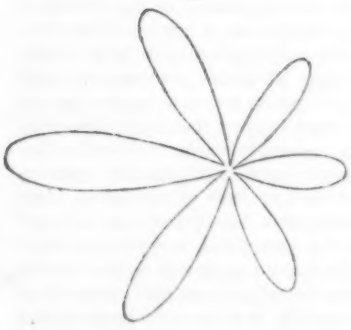
2.



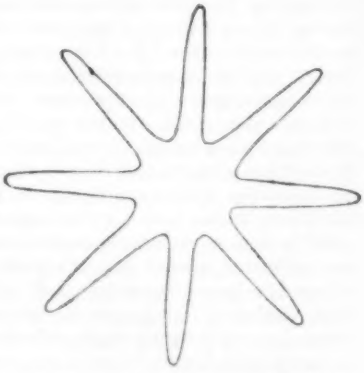
3.

4.

5.



6.



The equations for these curves are as follows. 1. $\varrho = \pm m \cos 3\theta$, (from the paper published in 1723 by Dr. Grandus, entitled "A Collection of Geometrical Flowers," Abr. Phil. Trans. vi, 67.) 2. $\varrho = \dfrac{4{\cdot}5}{1 + {\cdot}1 \cos 5\theta}$.

3. $\varrho = 4 + \cos 5\theta$. 4. $\varrho = 4 + \cos 10\theta$. 5. $\varrho = \dfrac{3\,65 + 3{\cdot}55 \cos 5\theta}{1 + {\cdot}3 \cos \theta}$.

6. $\varrho = \dfrac{3{\cdot}2}{1 + {\cdot}6 \cos 8\theta}$.

2. *Gyroscope.*—Major Barnard will continue his discussion of the gyroscope in the January number of this Journal, and treat especially of "the motions of the gyroscope as modified by the retarding forces of friction and resistance of the air,"—explaining the discrepancies between the theoretical motion of a "solid of revolution" without friction or other retarding influences and the observed motions of the experimental gyroscope.

3. *Rapidity of thought or nervous action;* by M. Ule, (Revue Suisse, March 15, 1857). The method of transforming the valuation of time into space by the rapid revolution of a cylinder, proposed by Mr. Fizeau, has been applied to the measurement of the rapidity of nervous impulse. Such a cylinder rotating 1000 times a second, and divided into 360 degrees, may measure 1–360,000th part of a second; or rotating 1500 times a second, 1–540,000th part of a second; and even this may be subdivided by a microscope, so as to obtain the 10-millionth or perhaps 100-millionth part of a second. By this extreme minuteness of subdivision of time, it is not difficult to measure even the rapidity of a nervous impulse. If an electric shock be given to the arm, it produces a sensation, and a contraction of the muscles. Hence by noting the interval of time between the shock and the contraction, the time occupied by the transmission of the sensation and the action of the brain, however quick, will be determined. By trying the experiment with different parts of the body, sensible differences have been observed, the shock applied to the thumb being one-thirtieth of a second behind that applied to the face; and this difference pertains to the transmission and not to the action of the brain, and hence enables us to eliminate the latter in the experiments. In this way it has been found by M. Helmholtz, by whom these experiments have been made with the most care,

(1.) That sensations are transmitted to the brain at a rapidity of about 180 *feet per second*, or at one-fifth the rate of sound; and this is nearly the same in all individuals.

(2.) The brain requires *one-tenth of a second* to transmit its orders to the nerves which preside over voluntary motion; but this amount varies much in different individuals, and in the same individual at different times, according to the disposition or the condition at the time, and is more regular, the more sustained the attention.

(3.) The time required to transmit an order to the muscles by the motor nerves, is nearly the same as that required by the nerves of sensation to pass a sensation; moreover it passes nearly one-hundredth of a second before the muscles are put in motion.

(4.) The whole operation requires 1¼ to 2 tenths of a second.

Consequently when we speak of an active, ardent mind, or of one that is slow, cold or apathetic, it is not a mere figure of rhetoric.

4. *Smithsonian Contributions to Knowledge*, Vol. IX, 4to.—Contents. —L. W. Meech: On the relative Intensity of the Heat and Light of the Sun upon different latitudes of the earth. 58 pp., with 6 plates.

Edward Hitchcock: Illustrations of Surface Geology. 164 pp., with 12 plates.

Brantz Mayer: Observations on Mexican History and Archæology, with a special notice of Zapotec Remains as delineated in Mr. J. S. Sawkin's drawings of Mitla, etc. 36 pp., with 4 plates.

W. Gibbs and F. A. Genth: Researches upon the Ammonia-Cobalt bases. 72 pp.

Appendix. J. D. Runkle: New tables for determining the values of the coefficients in the perturbative functions of planetary motion, which depend upon the ratio of the mean distances. Also, Asteroid Supplement to new tables for determining the values of $b_s^{(i)}$ and its derivatives.

The appearance annually of one or more quarto volumes of papers containing the results of recent researches is abundant testimony to the productiveness of the Smithsonian fund. This is but one of the ways, it should be noted, in which the Institution is successfully laboring to increase and diffuse knowledge. In all, it is doing a great work for the country.

The first paper by Mr. Meech is a mathematical investigation of the varying amount of heat received by the earth from the sun arising from the variations in her phases of position and inclination. The topics of the chapters will give an idea of the mode of treating the subject. 1. Irradiated surface upon the planets. 2. The sun's intensity upon the planets, in relation to their orbits. 3. Law of the sun's intensity at any instant during the day. 4. The sun's diurnal intensity. 5. The sun's annual intensity. 6. Average annual intensity upon a part or the whole of the earth's surface. 7. Secular changes of intensity. 8. Local and climatic changes. 9. Duration of sunlight and twilight. Under the 7th head, Mr. Meech deduces, that taking into view the fact that the obliquity of the ecliptic, 2000 years ago, in the time of Hipparchus, 128 B. C., was 23° 43′, and now it is 23° 27½′, and that therefore the sun then rose higher in summer than now, the summer heat of that period was *two-tenths* of a degree Fahrenheit hotter than that of the present, while the winter was the same amount colder.

The article by Prof. Hitchcock has already been noticed (see page 430), and that by Gibbs and Genth has been republished entire in this Journal.

5. *A Monograph of the Tertiary Entomostraca of England and Ireland;* by T. Rupert Jones, F.G.S. London, 1856, 70 pages 4to, with 6 plates.—This work is one of the monographs published in elegant style by the Palæontographical Society of London, one of the most useful Scientific publishing Societies in the world. Mr. Jones has paid great attention to these minute Cypris-like species, and his works embrace a large part of what is known on these fossils. The Quarterly Journal of the Geological Society of London for November, 1856, contains another paper by Mr. Jones, on the *Estheria minuta*, a triassic species of the same group of Crustacea.

6. *Terrestrial Magnetism at Brussels and Paris;* M. Mahmoud, (Bull. Acad. Roy. Belgique, xxii, 14).—Inclination obtained with a Gambey needle in the garden of the Observatory, May 10–18, 1855, 67°42′·82 (mean of four determinations); absolute horizontal intensity, 1·8075; absolute total intensity, 4·7662. At Paris, March 4 and 5, inclination 66° 23′·18, absolute horizontal intensity 1·8817, absolute total intensity 4·6976.

7. *Fishes of China.*—Volume X, of the Nouveaux Mémoires de la Soc. Imp. des Naturalistes de Moscou, 1855, contains a paper on the Fishes of Northern China, by Dr. S. Basilewsky, with 9 quarto colored plates.

8. *United States Japan Expedition;* by Commodore M. C. PERRY, U.S.N. Vol. II, 420 pp., 4to, with many maps and plates.—The first and third volumes of the Reports upon this expedition appeared some time since, and now the second is added. It is devoted to the agriculture, mineral and other resources, natural history, geological characteristics, etc., of the places visited by the squadron, especially of Lewchew, Peel Island, Formosa, Japan and China. Although there was no naturalist on board the vessels, many valuable facts were collected. Under natural history, there is an important paper by Mr. JOHN CASSIN, of Philadelphia, on the birds of Japan, with colored plates; notes on some colored figures of Japan Fishes, by J. CARSON BREVOORT; a list of shells collected in Japan, with 5 plates, by JOHN C. JAY; Notes on the plants, by ASA GRAY; on the Ferns, by DANIEL C. EATON; exploration of Lewchew and other places, by Rev. GEORGE JONES.

9. *Fossils of South Carolina;* by M. TUOMEY and F. S. HOLMES. Nos. 11 and 12. Charleston, S. C., 1856. pp. 79–104, 4to, with 4 plates.—These numbers (in one) of this fine work continue the descriptions and plates of the South Carolina Pleiocene fossils, and include species of Venus, Petricola, Tellina, Psammocola, Lavignon, Sinodesmia, Amphidesma, Donax, Mactra, Gnathodon, Solecurtus, Panopœa, Pholadomya, Solen, and Pholas. The plates, as heretofore, are excellent, and the work should have a place in every geological library.

10. *Specimen Tables Calculated and Stereomoulded by the Swedish Calculating Machine.* xviii and 50 pp., large 8vo, with a plate of the machine. London, 1857. Longman, Brown, Green, Longmans & Roberts. —This calculating machine, by George and Edward Scheutz, is the most wonderful work of mechanical and mathematical skill combined which has been constructed. One of the specimen tables is a table of logarithms of numbers from 1 to 10,000, "calculated, steromoulded, and printed by machinery," a method which precludes error in the tables. It is stated moreover that "in the next machine, the rate of working may easily be increased ten-fold so that twenty pages may be quietly *calculated* and *stereomoulded* whilst a compositor was merely "setting up" a single page. Various other specimens of mathematical calculations are given.

11. *On Fichtelite, a fossil Hydro-carbon, found in the Fichtelgebirge of North Bavaria.* Inaugural dissertation, prepared for promotion to the degree of Doctor of Philosophy, and addressed to the Philosophical Faculty of the Georgia Augusta University at Goettingen, by T. EDWARDS CLARK, B.S., of Cambridge, Mass. 32 pp., 8vo. Heidelberg, 1857.—This memoir, which adds to our knowledge of the species Fichtelite, both as to its crystallization and composition, we propose to cite from at another time.

12. *Astronomical observations, made under the direction of* M. F. MAURY, Lieut. U. S. Navy, during the year 1848, at the U. S. N. Observatory, Washington. Vol. IV. Published by authority of the Secretary of the Navy. 308 pp., 4to. Washington, 1856.—This volume of tables contains the results of observations with the transit instruments, mural and meridian circles and equatorial instrument; and also the mean right ascension of stars observed with these instruments, mean declination, mean

right ascension of sun, moon and planets observed with the west transit instrument; declination and semidiameters of the same as observed with the circles, and catalogue of stars observed in 1848.

13. *Annals of the Astronomical Observatory of Harvard College.* Vol. II, Part I. 136 pp., 4to, with numerous plates. Cambridge, 1857.—This part of the Cambridge Observatory Annals relates wholly to Saturn, and contains the observations made at the observatory by Wm. C. Bond, Director of the Observatory. The general results have been before the public for some time, and their high merit is well known. The observations are brought down to May of the present year. The series of plates contain 120 figures representing the appearances of Saturn and the ring at as many different times of observation.

14. *Patent Office Report*, 1856, CHARLES MASON, Commissioner of Patents. Volumes I, II, III, 8vo. Mechanics and one volume on agriculture.—The third volume, of 600 pages, is made up wholly of plates of the various machines, etc. patented at the Patent Office, descriptions of which are given in the first and second volumes. The agricultural volume contains many practical papers on agriculture, agricultural products and related topics, and is illustrated by several plates.

15. *Illustrated Annual Register of Rural Affairs for* 1858, with 130 engravings, Number four, by Luther Tucker & Son, Albany, 1858. Contains, besides other rural matters, neat plans of houses and grounds, trellices, etc.

16. *Illustrated Scientific and Descriptive Catalogue of Achromatic Microscopes manufactured by J. & W. Grunow & Co., New Haven, Conn.* New Haven. 8vo, pp. 104.—This work deserves honorable mention as a treatise of no small merit upon the general principles of optics involved in the construction of the microscope. This subject is considered under the following heads. Chapter I. Theory of the Microscope. Chapter II. Mechanical structure of the Microscope. Chapter III. Accessory apparatus. Chapter IV. Polarized light and its application to the Microscope. Chapter V. Practical directions. Eight distinct forms of the microscope, all original with these artists, are described and figured with their accessory apparatus. These are, No. 1, Educational microscope. No. 2, Students microscope. No. 3, Student's microscope. No. 4, Student's larger microscope. No. 5, Another form of student's microscope. No. 6, Portable microscope. No. 7, Large microscope. No. 8, Simple form of inverted microscope; to which may be added the more complete inverted microscope. The figures in this treatise are all original drawings by Sanford from the instruments, and are remarkably good examples of the xylographic art. These artists have attained a most enviable perfection in all the nicer details of construction in the mechanical portion of the instrument, while in the optical department they have also obtained the most satisfactory results. There are points of great interest to the microscopist discussed in their Catalogue which we may hereafter transfer to our pages. Such is § 124, on the "*Qualities of Object-glasses.*" Several novelties of construction are also for the first time here described and figured. The book is beautifully printed, and will be sent by post to any address upon the transmission of thirty cents in stamps to the artists.

17. *Annals of the Lyceum of Natural History of New York*, Vol. VI, Nos. 6 and 7, pp. 165–230. New York city: Wiley & Halsted. *Contents*—

G. N. Lawrence: Descriptions of new species of Birds of the Genera *Chordeiles* Swainson, and *Polioptila* Sclater. *Species*, Chordeiles Gundlachi from Cuba, C. Texensis of Texas, Polioptila melanura of Texas and California.

John H. Redfield: Descriptions of new species of North American Helicidæ.—Species, *Helix Elliotti* of the mountains of Georgia and North Carolina, and *H. barbigera* of Georgia.

J. T. Gulick: Descriptions of new species of Achatinella from the the Hawaiian Islands. Contains descriptions of 49 species, with figures on two plates.

18. *Supposed Meteorite.*—We have received from Mr. Thomas Bassnett of Ottawa, Illinois, specimens of scoria, with a description of their supposed fall on the 17th of June last, about ten miles south-southwest from that place. The account of the fall, as written out by L. H. Bradley on whose farm the scoria was found, states, that the time it occurred was fifteen or twenty minutes before 2 P. M.; the wind blew west by south. He says, "The cinders fell in a northeasterly direction in the shape of the letter V. The weather had been showery, but I heard no thunder and saw no lightning. There appeared to be a small, dense black cloud hanging over the garden in a westerly direction, or a little to the south of west. The cinders fell upon a slight angle within about three rods of where I was at work; there was no wind at the moment, or none perceptible. My attention was called first to the freak the wind had in the grass, and the next moment to a hissing noise caused by the cinders passing through the air. The larger ones were considerably imbedded in the earth, so much as only to show a small part of it, while the smaller ones were about one-half buried. I noticed at the time that the ground where I afterwards picked up the cinders showed signs of warmth, as there was quite a steam or fog at that particular point. I thought it singular, as the ground had been very cold previously."

The scoria is in rounded inflated pieces, like what have been called volcanic bombs, the exterior being glassy, and the interior very cellular. They are little over an inch in the longest diameter. Color black.

The paper, called the "Sunny South," of Aberdeen, Miss., of Sept. 17, 1857, describes a mass of lava as large as a barrel, "which fell near the farm of Mr. John Fortson, ten miles west of Aberdeen, on the 8th of July, 1856, and which excited a good deal of wonder and speculation at the time for hundreds of miles around."

The Illinois scoria is unlike any meteorite, and suggests the idea of a terrestrial origin. We know nothing about the Mississippi "lava."

Proceedings Boston Soc. Nat. Hist.—Vol. VI. p. 145, List and descriptions of eggs obtained in California; *E. Samuels.*—p. 145, On erosions of shells; *J. Lewis.*—p. 152, On a supposed new Siredon; *S. Kneeland.*—p. 155, On a new Succinea and Helix; *W. G. Binney.*—p. 157, On a new Guano; *A. A. Hayes.*—p. 158, Note on the wild turkey breeding with the tame; *Bryant.*—p. 160, A psychological view of the motions of Animals; *D. Weinland.*—p. 167, Cementation of Conglomerates; *A. A. Hayes.*—p. 173, 179, 194, Notice of J. W. Bailey; *A. A. Gould.*—p. 175,

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Proceedings of the Academy of Natural Sciences of Philadelphia. 1857.—p. 2, Catalogue of the species of Bembidium found in the U. States and contiguous regions; *J. L. LeConte.*—p. 10, Descriptions of several new Mammals from Western Africa (species of Semnopithecus, Cercopitheus, Microcebus, Vespertilio, Sorex, Sciurus); *John LeConte.*—p. 15, Notes on Aluminium; *W. J. Taylor.*—p. 17, On the visual organs of certain species of the genus Unio; *I. Lea.*—p. 18, Note on Entozoa in the Naiades; *J. Leidy.*—Descriptions of American Land Shells; *W. G. Binney.*—p. 19, New species of invertebrates of N. Pacific Expedition; *W. Stimpson.*—p. 31, Descriptions of thirteen new Unios from Georgia; *I. Lea.*—p. 33, Catalogue of Birds collected on the river Muni, W. Africa; *J. Cassin.*—On the Crania of the Ancient Britons, with remarks on the people themselves; *J. B. Davis.*—p. 48, Reptiles of the Gaboon country, Western Africa; *E. Hallowell.*—p. 75, Synopsis of the genus Clivina and allied genera inhabiting the U. States; *J. L. LeConte.*—p. 83, Description of six new species of Unios from Alabama and of species of U. S. Naiades; *I. Lea.*—p. 87, Contributions to the Neuropterology of the U. States; *P. R. Uhler.*—p. 89, List of extinct Vertebrata, which have been discovered in the region of the Missouri River, with remarks on their Geological age; *J. Leidy.*—p. 91, On Columbian Guano; *W. J. Taylor.*

Magnetical and Meteorological Observations, made at Toronto in Canada; printed by order of her Majesty's Government, under the superintendence of Major-General Edward Sabine of the Royal Artillery. Vol. III. 1846, 1847, 1848, with abstracts of observations to 1855 inclusive. 456 pages 4to. London, 1857. Longman, Brown, Green & Longmans.

Mémoires de l'Académie Royale des Sciences des Lettres, et des Beaux Arts, de Belgique. Tome xxx, 1857. Contains, in science, Experimental and Theoretical researches on the figures of equilibrium of a liquid mass without weight, by *J. Plateau.*—Problem "des Crepuscules," by *Mr. Laigre.*—Influence of the lunar changes on the atmospheric pressure, *ibid.*—Fundamental notions on several elementary points in Geology, dynamics and transcendental Analysis, by *M. Lamasle.*—Researches on the development of Infusoria, by *M. J. d'Udekem.*—Observations on Periodical Phenomena.

Memoires Couronnés, ibid.—Vols. xxvii, xxviii. Scientific Papers: Development of the Lumbricus terrestris; *M. J. d'Udekem.*—Researches on the specific heat of some metals at different temperatures; *E. Bede.*—Cause of the scintillation of the stars; *Montigny.*

Bulletin of the Acad. Sci. Roy. of Belgium. Vol. xxii, contains the following papers in Zoology and Botany: Ichneumonologica Miscellanea; by *Prof. C. Wesmael* of Brussels.—On Cecrops Latreillii; *van Beneden.*—New species of Scolex; *J. d'Udekem.*—New classification of the setigerous Abranchiate Annelida; *J. d'Udekem.*—New species of Michelaria; *Strail.*—On the species of Hirundo, of the subgenus Cecropis; *E. de Selys-Longchamp.*—Vol. xxiii. On the Indigenous varieties of the Fucus vesiculosus; *J. Kickx.*—Additions to the Recapitulation of the hybrids observed in the family of the Anatida (see vol. xii, No. 10, p. 335, 1845); *Selys-Longchamps.*

Faune Française, ou Histoire Naturelle, Générale et Particulière des Animaux qui vivent en France; par Charles-Lucien Bonaparte et Victor Meunier.—A prospectus of this great work has just been issued. It is proposed to extend it to 24 volumes, 8vo, with a large number of illustrations, and to publish one volume every three months, commencing with the first of January, 1858.

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